

# Psychological Review

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# THE PSYCHOLOGICAL REVIEW

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## THE CASE FOR THE TOLMAN-LEWIN INTERPRETATION OF LEARNING

BY RALPH K. WHITE

*Cornell University*

### I. INTRODUCTION

There are at least four criteria by which a set of scientific postulates can be judged:

1. *Operational meaning*.—Are the postulates definite enough so that concrete experimental predictions can be based upon them? If so, then agreement between divergent 'points of view' can be achieved: (a) By recognizing as merely terminological all those differences which do not make a difference experimentally; and (b) By submitting to experimental test the differences which do make a difference experimentally.

2. *Rigor*.—Is there an unbroken sequence of explicit logical steps between the concrete experimental conditions, which are used as a starting-point for prediction, and the predictions which are made? Or are there unacknowledged gaps where 'intuition' is permitted—perhaps unintentionally—to take the place of explicit logic?

3. *Economy*.—Is there a maximum of diversity in the concrete facts which can be deduced or predicted on the basis of the postulates, combined with a minimum of complexity in the postulates themselves? Is there economy or parsimony, in the sense that a maximum of predictive power can be purchased with a minimum of theoretical outlay?

4. *Experimental basis*.—Have the facts which are deducible from the postulates been solidly established on the basis of extensive and well-controlled experimentation?

This paper is an attempt to show that the Tolman-Lewin interpretation of learning measures up to all four of these criteria, and that it is superior to S-R psychology in regard to

the third and fourth criteria, 'economy,' and 'experimental basis.'<sup>1</sup> It is not claimed that there is anything original or new in Tolman's or Lewin's most basic assumptions. It is claimed that two of their most basic assumptions—'perceptual learning' and 'path-goal behavior'—are the indispensable foundation of any sound learning theory.<sup>2</sup>

## II. OPERATIONAL MEANING

1. *Agreement with 'common sense' and disagreement with S-R psychology.*—The question of operational meaning is especially pertinent because many persons have doubted whether, underneath the highly elaborate terminologies of Tolman and Lewin, there is any clearcut and distinctive meaning at all.<sup>3</sup> There are really two questions here: (1) Is the Tolman-Lewin approach merely a pair of vague and pretentious terminologies, or does it embody some definite, intelligible assumptions about the learning process? (2) If there are any definite assumptions, are they not already accepted by all present-day psychologists, including S-R psychologists, so that there is no need to dwell upon them? Are the differences not purely terminological?

It will be our first task in this paper to answer these questions as simply and intelligibly as possible. We will try to show: (1) that underneath the luxuriant terminological wilderness there are at least two perfectly definite assumptions; and (2) that, while these assumptions are simply an explicit statement of 'ordinary common sense,' they are by no means fully and explicitly accepted, as they should be, by all present-day psychologists. They are not accepted by the consistent S-R

<sup>1</sup> For the clarification of the criteria the writer is indebted primarily to Hull. Hull's logico-empirical method and his psychological premises are, however, two different things. By using Hull's own method, the writer has been led to the conclusion that Hull's psychological premises are scientifically inadequate.

<sup>2</sup> The term 'field theory' might have been used to represent these two assumptions. It has been avoided here, primarily because of the semantic difficulties involved in a word which has been used with so many different connotations (including emotional connotations) by so many different persons. The term 'Tolman-Lewin interpretation' has a relatively definite meaning: the assumptions which are held in common (though expressed very differently) by Tolman and Lewin.

<sup>3</sup> For instance, Hull, quoted by Hilgard and Marquis (8, 254), Householder (9), and Young (31, 291).



psychologists. Specifically, they are not accepted by Thorndike or by Hull. They are distinctly incompatible with at least some applications of the S-R principle which Thorndike calls the 'law of effect' and which Hull calls the 'principle of reinforcement.' They can be best described, in fact, as a return to common sense—on a scientific level—at certain points where the consistent law-of-effect psychologists had departed from it.

To make more concrete this crucial distinction between S-R psychology, on the one side, and Tolman and Lewin and 'common sense' on the other, we will begin by describing a crucial experiment, in which the two sets of postulates lead to diametrically opposite predictions. It is the sort of experiment the value of which has been repeatedly emphasized by Hull in particular—the sort of experiment that shifts a basic theoretical issue from the level of endless verbal disputation onto the level of solid experimental fact.<sup>4</sup>

A hungry person is repeatedly placed in a simple two-alternative choice situation. The alternatives are two paths, *B* and *C*. One of them, *B*, leads to water, and the other, *C*, leads to food. Neither the food nor the water can be seen, smelled, or otherwise perceived by the person at the choice-point. But naturally, after exploring both paths, he quickly 'learns' to take the right-hand path whenever, being hungry, he enters the choice situation. The great question, which for the moment we will leave open, is the question of *how* he learns it. For the present we will simply note two objective facts: the person has fully explored both pathways, with ample opportunity to perceive the water in one and the food in the other; and, being hungry, he has come to choose rather consistently the right-hand path.

All of the above is preliminary to the decisive part of the experiment. The decisive part comes when, for the first time, the person is made thirsty instead of hungry. Instead of being deprived of food for twenty-four hours before the ex-

<sup>4</sup> For the original plan of this experiment the writer is indebted to Professor Kenneth Spence, who, with Dr. Lippitt, carried out a similar experiment with rats (21). The writer has carried out an analogous experiment with human subjects; for the results, see below, pp. 180-181.

periment, he is given plenty of dry food but no liquids. Which path will he now take? That is a cleancut operational question.

It is here suggested that a completely consistent S-R psychologist, whose assumptions are as explicit and whose logic is as clear as that of Hull, would necessarily say, 'the right-hand path.' Sheer habit, of course, would favor this response, because it is the response which has been most often exercised. More important, however, from the standpoint of both Thorndike and Hull, is the fact that this right-turning response is

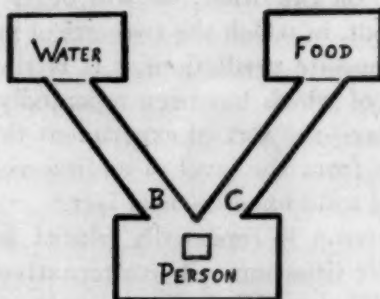


FIGURE. The person has often had an opportunity to perceive that *B* leads to water and *C* leads to food. In the past he has always been hungry and has consequently 'learned' to take path *C*. He is now thirsty instead of hungry. Which path will he now take?

also *the only response which has ever been rewarded*. The person has repeatedly gone to the right and, each time, he has been rewarded by the presumably satisfying experience of eating-food-when-hungry. The connection leading to this response should, then, have been repeatedly 'reinforced.'<sup>5</sup> In

<sup>5</sup> The apparent 'stupidity' of this response (going to the right—toward food—even when thirsty and not hungry) may lead some readers to question whether such a prediction does follow inevitably from the law-of-effect assumption. Such readers may ask whether it is not true that both Thorndike and Hull now 'take motivation into account,' and whether, when motivation is 'taken into account,' the prediction would not be reversed.

The question is a legitimate one, and can be fully answered only by showing that, even when motivation is taken into account, the Thorndike-Hull assumption would still lead to the same prediction. For readers who are familiar with Hull's notation, the following analysis is presented:

Let the choice-point situation be represented as  $S_1$ ; let the motive or drive-stimulus of hunger be represented as  $S_H$  and the drive-stimulus of thirst as  $S_T$ . Let the right-turning response be represented as  $R_R$  and the left-turning response as  $R_L$ . It can

the crucial trial, it should be stronger than any competing S-R connection, and the person should go to the right.<sup>6</sup>

On the other hand, the person *wants water*, and he *knows where it is*. (He knows where it is, because he has often seen it there.) Is there any doubt about the prediction that would be made by a naive, psychologically unsophisticated person? Is there any doubt that a naive person would say, "The person will go to the left"?<sup>7</sup>

The immediate point at issue, it should be remembered, is not whether the S-R psychologist or the naive person is correct. The question is whether any assumptions can be set up, as an alternative to the S-R psychologist's assumption of 'reinforcement,' *which differ from that assumption in a perfectly clearcut way*. It should be clear by now that some other assumptions are possible—assumptions which differ from the S-R assumption 'operationally,' to the extent that they lead to a different concrete prediction. It should also be clear that these assumptions, whatever they are, have an eminently 'common-sense' character; they are the sort of thing which ordinary people take completely for granted. But that is not enough. Hull has rightly pointed out that even a correct conclusion, arrived at by 'intuition' or deduced in a slipshod fashion from vaguely worded premises, has little or no scientific value. It may prove something, but *what* does it prove? If Tolman and Lewin are to lay claim to any contribution other than a vague agreement with equally vague common-sense assumptions, their contribution must include, first of all, such an explicit statement of their own premises that the

then be said that, at the moment when reinforcement occurs, two connections are reinforced: the connection  $S_1 \rightarrow R_R$  and the connection  $S_H \rightarrow R_R$ . These two connections converge upon the response  $R_R$ . When the motive is changed from hunger to thirst, the second connection,  $S_H \rightarrow R_R$ , becomes inoperative because  $S_H$  has disappeared. However, the connection  $S_1 \rightarrow R_R$  still remains, and it should be a strong one because it has been so often reinforced. Balanced against it there might conceivably be a connection  $S_T \rightarrow R_L$  (operating directly or by way of 'anticipatory reactions'), but this connection could not be a strong one, because it has never been reinforced. One strong connection, then, tends to evoke  $R_R$ ; a weak connection or no connection leads to  $P$ ; hence,  $R_R$  should occur.

<sup>6</sup> See Thorndike (23, 176; 24, 205) and Hull (10; 14, 821).

<sup>7</sup> Anticipating our later discussion on pp. 180-182, we can say that the experimental data support the naive person's prediction—on the human level if not also on the rat level. That, however, is not now the point at issue.

agreement with common sense (and the exact nature of the disagreement with S-R psychology) is absolutely clear. The first contention of this paper is that they have done so—if not with absolute clarity, at least with a degree of clarity sufficient for ordinary psychological discussion and experimental prediction.

The difficulty, of course, is to discuss either Tolman's or Lewin's postulates in an article of this length without devoting an impossible amount of time to questions of terminology. As an admittedly inadequate makeshift, therefore, we have attempted to express two of their postulates with the greatest possible amount of simplicity, using the sort of everyday words which might be immediately intelligible to a person unfamiliar with their terminology, even though this means some sacrifice of the full meaning which the postulates have in their original form.

The first postulate can be called the 'perceptual-learning postulate.' In the context of this experiment, it means that the person can learn by merely *seeing* the water at the end of the left-hand path—by merely perceiving it in that place. In a more generalized form, this postulate might be stated as follows: *When a particular piece of behavior in a particular situation is once perceived as a path to a particular object (e.g., when the act of entering B is perceived as a 'path to,' or way of obtaining, water<sup>8</sup>), a more or less permanent 'knowledge' of this relationship usually results. As a rule this knowledge is available to the organism when the same situation is again presented.* (For instance, when he is again at the choice-point, the person 'remembers' that the act of entering B is a path to water.)

This formulation is of course a gross oversimplification of two extremely complex problems—the problem of learning and the problem of recall or utilization of acquired knowledge. It is the sort of oversimplification in which neither Tolman nor Lewin would ever indulge. It is a rough translation, however, of Tolman's assumption that perception builds up

<sup>8</sup> Note that the anticipated act of entering B, rather than the region B itself, is here spoken of as a 'path.'



'sign-gestalt expectations' (25, 134-192), and of Lewin's assumption that perception changes the 'cognitive structure of the psychological environment' (18, 71-72, 133, 218). Obviously, all of these propositions have a common core of meaning, and all of them are in essential agreement with the tacit assumptions of common sense.

The second or 'path-goal postulate' has an equally common-sense character. In our experiment it means that if the person wants water, and if he remembers that *B* is a path to it, he is likely to enter *B*. In a more generalized form it might be stated as follows: *If there is a motive or 'need,' the goal of which is a particular object (e.g., if the person has the motive of thirst, which means that he has a need for water or that his goal is water), and if at the same time there is available the knowledge that a particular piece of behavior is a path to that goal-object (e.g., if he knows that the act of entering B is a path to, or way of obtaining, water), then that behavior will tend to occur.*

This is a rough translation of Tolman's postulate, which in this context implies that if a person has a 'sign-gestalt expectation' to the effect that 'commerce with' *B* will lead to water, and if he has a 'demand' for water, he is likely to enter *B* (25, 94; 26, 202). It is also a rough translation of Lewin's postulate which implies that if *B* is a path to water, in the 'cognitive structure of the person's psychological environment,' and if water has a 'positive valence,' there will be a 'force' acting upon the 'person' and tending to make him enter *B* (19, 27, 88-90). Here again, despite great differences in terminology, all three of the propositions mean essentially the same thing, and they all are in essential agreement with the commonest sort of common sense.

2. *Four differences between the perceptual-learning postulate and S-R psychology.*—The first reaction of an S-R psychologist to the perceptual-learning postulate, as we have restated it, might well be: "But this is S-R psychology. The 'acquisition of knowledge' is nothing but the acquisition of new S-R connections. In this experiment, the sight of the path *B* is the

stimulus, and an idea of water—Hull would call it an anticipatory reaction to water—is the response.”

There is so much real similarity between this kind of S-R interpretation and the Tolman-Lewin interpretation that perhaps, for the sake of harmony, it would be better to stress the core of meaning which they have in common rather than stressing their differences. Certainly this common core of meaning has not been enough emphasized; both the S-R psychologists and the ‘field theorists’ have often failed to realize the extent to which they were actually saying the same thing in different words. At the same time, there are some real differences; and since our primary purpose here is to demonstrate that the Tolman-Lewin interpretation does differ from S-R psychology, these differences must be clarified. They can be summarized as follows:

(1) The perceptual-learning postulate *refers only to the acquisition of ‘knowledge,’* and not, as S-R psychology does, to overt responses also. ‘Perceptual learning’ implies a sort of association between ‘ideas,’ but not between a situation and an overt act. In our experiment, this means that the person at the choice-point will probably ‘know about’ or ‘remember’ the water at the end of the path; nothing whatever is implied about an actual tendency to enter this path.

Both Tolman and Lewin make a fundamental distinction between knowledge (‘sign-gestalt-expectations,’ or ‘connected regions in the psychological environment’) and overt behavior.<sup>9</sup> Hull and many other S-R psychologists make a somewhat analogous distinction, but they have never postulated that the principle of association is confined to ‘ideas’ or ‘anticipatory reactions.’ Tolman, on the other hand,

<sup>9</sup> The motor theory of thought, which postulates that an idea is itself a motor act, is not here explicitly contradicted; but it is at least definitely assumed, by both Tolman and Lewin, that functionally there is a radical difference between knowledge and the gross movements which are ordinarily observed.

Incidentally, the Tolman-Lewin interpretation (although it is strictly and humbly non-physiological) does imply certain characteristics which a genuinely explanatory physiological theory of behavior—if and when it finally emerges—must possess. Such a theory must, first of all, describe and differentiate the physiological characteristics of ‘needs’ and ‘knowledge,’ in such a way as to account for the well-established ‘multiplication’ relationship between them (defined below, pp. 169-172).

while he comes very near to S-R psychology by explicitly accepting the 'association of ideas' (as Lewin does not), rejects association as a direct explanation of overt actions (25, 203). As he points out, this is in a sense a return to eighteenth-century associationism, which (like common sense) talked about the 'association of ideas,' but which also (like common sense) retained 'ideas' as a necessary intervening factor between the externally-observable stimulus and the externally-observable response.<sup>10</sup>

(2) The knowledge to which the perceptual-learning postulate refers is not composed of isolated ideas or anticipatory reactions, but of *patterned knowledge*, containing at least two parts or 'regions' which need to be specified, and a specified relationship between them. In our example, the postulate does not imply merely that perception of *B* evokes an 'idea' of water. There is something more than a perception of *B* and then, separately, an idea of water. There is an integrated psychological process in which the 'perception' of *B*, the 'idea' of water, and the 'leading to' relationship are simultaneously present—a piece of patterned knowledge which can only be characterized as '*B*-leading-to-water,' or '*B*-as-a-path-to-water.' Perception of *B* reactivates, to some degree, the total perceptual configuration of which the perception of *B* was, originally, only a part. It is redintegration rather than mere association.

Obviously there is an application, here, of Gestalt theories of perception. Another way of putting it is to apply the familiar characterization of all Gestalt theories of learning, and to say that the Tolman-Lewin interpretation implies that even learning which is usually called 'associative' actually implies 'insight.' Insight, however, is a highly ambiguous term, requiring careful definition. If it is used at all in this context it should be definitely understood as synonymous with patterned knowledge, and not necessarily as implying the sudden acquisition of patterned knowledge. Perceptual learning may be sudden or it may be very gradual—as Köhler and Koffka have both pointed out. In this context, insight can-

<sup>10</sup> *Ibid.*

not possibly be defined as a 'sudden drop in the learning curve'; to do so means to obscure the genuine difference between a system which assumes patterned knowledge, as an essential link in its logic of prediction, and a system which does not.<sup>11</sup>

(3) The perceptual-learning postulate implies the importance of *perceptual 'field' conditions at the time of the original perception*, rather than any subsequent reward or 'reinforcement.' This difference is both an affirmation and a denial. It affirms the importance in relation to learning, and not merely in relation to perception, of all those field conditions which have been experimentally shown to influence perceptual organization: temporal contiguity, spatial contiguity, visual continuity, common contrast, embeddedness, exploratory motivation, etc. All of these factors, except temporal contiguity, have been given less emphasis by S-R psychologists. At the same time, it denies the direct importance of the reward or reinforcement factor, upon which many S-R psychologists, including both Thorndike and Hull, have laid a great deal of emphasis. In our experiment the operational meaning of this difference is clearcut. Tolman or Lewin would predict that variations in perceptual conditions, such as the degree of visual continuity between the entrance to path *B* and the water-box, would influence the learning of the '*B*-leading-to-water' relationship; Hull, on the basis of his present postulates, would not. On the other hand, Tolman or Lewin would not predict improvement in this learning (providing that exploratory motivation was high at the time of the original

<sup>11</sup> It is doubtful whether any S-R psychologist has ever denied the possibility of patterned ideational responses, but such responses do not yet constitute an essential link in the S-R psychologist's logic of prediction. There is nothing in any of Hull's diagrams, for instance, which would adequately characterize what we have called 'knowledge-of-*B*-as-a-path-to-water,' and which might be symbolized as '*b-w*,' using small letters to differentiate it from the actual path *B* and the actual water *W* in the objective environment of the organism. Hull has often diagrammatically represented 'anticipatory reactions' which can readily be compared with our term 'knowledge' if the adjective 'perceptual' is added. That is to say, the nearest Hullian equivalent of the term 'knowledge' would be 'anticipatory perceptual reaction.' His anticipatory reaction, however, has always had just one subscript. It has been '*r<sub>0</sub>*,' not '*r<sub>B-G</sub>*' or '*r<sub>B-W</sub>*.' As long as this limitation of his diagrammatic technique continues, it is difficult to see how he could strictly parallel the Tolman-Lewin logic of prediction with any logic of his own.



perception) if it were followed by a reward or reinforcement situation; Hull or Thorndike would.<sup>12</sup>

(4) The perceptual-learning postulate implies also the importance of '*field*' conditions at the time of recall. The postulate states that 'as a rule' the knowledge acquired through perception is available to the organism when the same situation is again presented. The phrase 'as a rule' was intended to suggest what Tolman and Lewin both explicitly state, namely, that the redintegration of the original perception depends upon several factors in addition to the mere reoccurrence of part of it.

Lewin has gone so far as to deny that an association can be a 'driving force' at all. As he has put it (17, 44-46), an association cannot be the 'motor' or source of energy for a 'psychic event.' It can act as a 'restraining force'; it can determine 'directions,' or 'steer' a psychological process, but it cannot, by itself, be a cause of such processes. It is doubtful whether Tolman, with his explicit acceptance of the 'association of ideas,' would go as far as Lewin does in this respect. Since it is not necessary, in any case, for our own logic, it has not been explicitly included in our statement of the postulate. In practice, however, it has some implications the importance of which should not be minimized. It is true that many S-R psychologists now grant a considerable amount of importance to such a factor as the 'mental set' at the time of recall; but the degree of their emphasis is not usually as great as Lewin's. Zeigarnik's data on the role of 'tensions,' as one field condition at the time of recall, are now well known in the United States.<sup>13</sup> Probably a more fundamental experiment (which unfortunately has not been translated into English<sup>14</sup>) is that of Lewin himself which indicates that the

<sup>12</sup> This is not equivalent, as some S-R psychologists have supposed, to an assertion that perceptual learning can occur apart from any sort of motivation. 'Incidental' learning plays no more important a role in the Tolman-Lewin interpretation than in any other interpretation which is well grounded in the experimental data. It is quite possible that motivation of some kind—e.g., an 'exploratory drive' or 'need for orientation'—is always present whenever perceptual learning is 'attentive' enough to be reasonably efficient. The decisive factor, however, is motivation at the time of perception, not a reward which occurs later.

<sup>13</sup> For a condensed report in English, see (5).

<sup>14</sup> For a very condensed report in English, see Thorndike (23, 431-436).

effect of a thoroughly habitual association may be reduced practically to zero if there is a deliberate intention to behave in a different manner.

3. *One crucial difference between the path-goal postulate and S-R psychology.*—It will be recalled that the path-goal postulate was stated as follows: "If there is a motive or 'need,' the goal of which is a particular object, and if at the same time there is available the knowledge that a particular piece of behavior is a path to that goal-object, that behavior will tend to occur."

The first reaction of an S-R psychologist might be: "But this is S-R psychology too. I now give a great deal of emphasis to needs or motives—Hull would call them drive-stimuli and anticipatory goal reactions—and as for 'goal-seeking behavior,' which Tolman and Lewin describe rather vaguely, I can actually explain it by the conditioning of adaptive responses to drive stimuli." (For Hull's more detailed statement of this explanation, see 10, 11.)

Here again there is some real similarity between the 'field-theoretical' and the S-R interpretation; and again, for the sake of harmony, it is well worth while to emphasize the common core of meaning. Certainly it should be recognized that both interpretations lay much stress upon motivation, and that both stress also the obvious fact that organisms very often exhibit a preference for behavior which has been followed by a reward situation. In our experiment, for instance, both interpretations would imply the 'learning' of the right-hand turn when the hungry organism finds food after making this turn, and when the organism continues to be hungry. Again, however, there are significant differences, and only obscurity can result from ignoring them. The mere fact that the two postulate-sets lead to different predictions, in the second part of our illustrative experiment, should put the clear-thinking S-R psychologist on his guard against a too easy identification of his motivation concept with that of Tolman and Lewin.

One obvious difference is that 'patterned knowledge' of a path-goal relationship is an essential part of the path-goal

postulate, while, as we have already noticed, it is not an essential part of the logic of prediction in any current S-R psychology. There is also an even more crucial difference:

The path-goal postulate implies that 'needs' and 'knowledge' constitute an *interdependent pattern* or 'field,' in at least this sense: that *either one without the other would have no appreciable tendency to produce this particular overt piece of behavior*. Neither one of them is a 'stimulus' to the overt behavior *B*, in the sense that it alone would have even a tendency to evoke *B*. It is not enough to concede that motivation may 'facilitate' an S-R connection, or that there may be connections between drive stimuli and responses. Tolman and Lewin do not postulate that overt behavior is determined by any S-R connections whatsoever. In their interpretation, the only factor which directly determines overt behavior is a 'psychological field,' defined as a mutually interdependent combination of at least two psychological factors: needs and knowledge.<sup>15</sup>

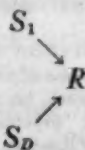
<sup>15</sup> Before elaborating the meaning of the word 'interdependent' in this definition, it will be worth while to clarify and emphasize another word—the word 'psychological.' There has been much confusion as to what Lewin means by the word 'field.' As we have just used the term it refers to a combination of factors, needs and knowledge, which are psychological in the sense that they are conditions or processes going on *inside the organism*. (It is not assumed that they are always 'conscious.') Certainly they do not exist *outside the organism*. On the other hand, most of Lewin's critics, and some of his uncritical followers, have assumed, not unnaturally, that when he speaks of the psychological field as being composed of 'person and environment' (19, 219), he is thinking of the environment as outside the organism. In fact, much of their criticism has been based upon this assumption. They have often inferred that when Lewin talks about a 'field' he is merely repeating, in a somewhat obscure and pretentious terminology, the truism that "there is interaction between organism and environment."

Actually Lewin's use of the term is essentially in harmony with our use of it here. The confusion has arisen primarily because of the unusual meaning which he has given to the word 'environment.' Even when he uses this term without the adjective 'psychological,' it always means the 'psychological environment'—a term which, translated rather loosely into ordinary English, means a person's *mental picture* of the world around him (18, 68-75). Leaving all epistemological issues aside, there is surely a genuine operational difference between a particular person's mental picture of his world, at a particular moment, and the external world as it 'actually exists.' Especially in abnormal and social psychology, where a person's picture of his world is often found to be extremely inadequate or distorted, the distinction is a fundamental one. Here again Tolman and Lewin are in agreement with the best sort of 'common sense' (including the best clinical theory and practice), but are not in agreement with the traditional types of S-R theory. The assumption that there is a patterned 'mental picture of the world' is a natural enough assumption, but Hull has not yet made use of it.

We have said that needs and knowledge are 'interdependent.' The grounding of this assumption in everyday experience can be seen in the case of our illustrative experiment. If the person in that experiment has no desire for water, then, surely, no matter how thoroughly he 'knows' that *B* is a path to water, there is no reason whatever to predict even a tendency to enter *B*. The knowledge of *B*-leading-to-water is not, by itself, a 'stimulus' to the behavior *B*. Similarly, if the person does not 'know' that *B* is a path to water, then no matter how much he wants water there is no reason to predict a tendency to enter *B*. The desire for water is not, by itself, a 'stimulus' to the behavior of entering *B*, any more than the knowledge is. Both the need and the corresponding kind of knowledge must be present, or the behavior *B* will not occur.

Since this is probably the most crucial difference between the two interpretations, it will be worth while to express it also in a diagrammatic and semi-mathematical form.

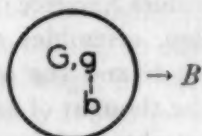
According to the S-R psychologists who stress the 'conditioning of adaptive responses to drive stimuli,' a response is usually the result of at least two main types of stimuli acting together: internal stimuli (including motives or 'drive stimuli'), and external stimuli impinging upon the sense organs. Following Hull, we may use the symbol  $S_1$  to represent some particular kind of external stimulation, and  $S_D$  to represent an internal drive stimulus. A formula of almost a minimum degree of complexity would then be:



Characteristically, though not necessarily, the two S-R connections evoking the response, *R*, are thought of as combining their strength in an additive manner, just as two physical forces, pushing in the same direction, would combine to produce a resultant equal to the sum of the two forces taken separately:  $R = f(S_1 + S_D)$ .



The simplest sort of Tolman-Lewin diagram might be the following:



In this diagram the behavior  $B$  is represented as being produced by a particular patterned combination of coexisting factors (the 'need-knowledge field') within the organism. This pattern is ordinarily composed of at least two components: a motive or need which can be designated by specifying its goal,  $G$ ; and a patterned piece of knowledge,  $g - - b$ , representing at least two intimately integrated parts,  $g$  and  $b$ , one of which corresponds to the goal  $G$ , and one to some particular piece of behavior,  $B$ . This piece of patterned knowledge,  $g - - b$ , is of course the knowledge which represents  $B$  as a 'path to' or a 'way of obtaining' the goal  $G$ . The circle and the single arrow mean that the whole field, rather than the separate parts which make it up, tends to produce the behavior  $B$ . And, since this behavior is not necessarily a 'response' to any 'stimulus,' it is called simply behavior,  $B$ , rather than a response,  $R$ .

One resemblance between this and the S-R diagram, which should be stressed more than it usually has been, is that both involve some sort of motivation concept. Hull's  $S_D$  corresponds to  $G$  in this diagram. There are, however, two significant differences. The piece of patterned knowledge,  $g - - b$ , is by no means identical with Hull's  $S_1$ ; and—even more crucial—the relationship between  $G$  and  $g - - b$  is more like multiplication than like addition. Speaking very roughly, the likelihood that the behavior  $B$  will occur is dependent upon the strength of the need or desire for  $G$ , multiplied by the degree of certainty with which the person 'knows' or 'thinks' that  $B$  is a path to  $G$ . If either factor is reduced to zero, their product is zero.  $B = f(G)(g - - b)$ .

In this respect the physical analogy of two forces pushing in the same direction, which was appropriate in the case of

the S-R formula, is no longer appropriate. If any physical analogy is to be used at all, a more appropriate one would be that '*G*,' the need, resembles a source of energy, while '*g* - - *b*,' the path-goal knowledge, resembles a channel or means of communication between *G* and the overt behavior *B*. The knowledge, *g* - - *b*, can be thought of as 'canalizing' or 'giving an outlet to' the energy obtained from *G*. Such an analogy would at least correspond to the fact which has just been mentioned—the fact that the absence of either the need or the path-goal knowledge corresponding to it will reduce their combined effectiveness to zero.

If there is any further doubt in the mind of a skeptical S-R psychologist as to whether this formulation is significantly different from his own, he might examine again our illustrative experiment and note how different the operational implications are from those of his own assumptions, *after* the need changes from hunger to thirst. His own concept of adaptive responses being 'reinforced' by a reward situation, and 'conditioned to' a drive stimulus, would indeed lead to a valid prediction of the right-turn-when-hungry. It would not, however, if rigorously applied, lead to the prediction of the left-turn-when-thirsty. This left-turning response could not have been previously 'reinforced' more than the right-turning response; nor could it have been 'conditioned to' the drive stimulus of thirst, because this drive stimulus did not exist at the times when this response had previously occurred.

The same analysis should dispose of the consoling hypothesis that the two interpretations are not in conflict because they are operating on essentially different 'levels'—the Tolman-Lewin interpretation on a more molar or macroscopic level, and the S-R interpretation on a more molecular or microscopic level. In the context of this experiment, at least, the two interpretations are operating on exactly the same level. The prediction of the right turn is on exactly the same level as the prediction of the left turn. And, on this level, the two predictions are diametrically opposed.

## III. RIGOR

1. *Can rigorous logic make use of a subjective terminology?*—

On the ground of rigor also the highly skeptical S-R psychologist is likely to have something to say. Hull, at least, has often thrown out this challenge; he would say, "Is it possible to have logical rigor as long as you persist in using outmoded subjective terms such as 'knowing' and 'wanting'? Isn't that just the sort of thing that scientific psychologists have been trying to get away from? Vague subjective terms of that sort are, apparently, always bound up with an intuitive rather than a scientific type of thinking."

The challenge is a legitimate one, because there can be no doubt that a subjective terminology has often been employed by speculative psychologists who had no apparent desire for logical rigor, and who preferred intuitive hunches ("What would I do if I were the rat?") to the more arduous and self-disciplinary processes of step-by-step logic. To some extent the same thing can perhaps be said about both Tolman and Lewin. While both of them have made significant contributions to the difficult problem of how 'needs' and 'knowledge' can be reliably inferred, neither of them has equalled Hull's strenuous efforts to make completely explicit every step in his own logic. Neither of them has ever stated, in a concise and systematic way, what his most essential postulates are, what the crucial terms in these postulates are, and how these crucial terms can be rigorously defined without using other equally unfamiliar terms in the process of definition.

It is not enough, therefore, to answer the S-R psychologist's challenge by saying that any terms are scientifically legitimate *if* they are defined in terms of concrete, non-intuitive, completely observable data. In all probability the S-R psychologist would grant this, as a methodological principle, but would simply question whether it had ever been done, or could be done, with 'subjective' terms. And the only answer to that is to do it—more clearly and completely than Tolman or Lewin ever has.

An article of this length is of course not the place to attempt a clarification of the complete Tolman-Lewin logic. It

is to be hoped that Tolman or Lewin will do so. We can, however, defend the legitimacy of the two terms 'need' and 'knowledge' by suggesting how they may be defined in terms of concrete observable data.<sup>16</sup>

2. *Operational definitions of 'need' and knowledge.*—A critical re-examination of our illustrative experiment should bring out the possibility of setting up at least two concrete techniques by which each of the two crucial concepts, 'need for water,' and 'knowledge of *B* as a path to water,' can be inferred. The inference that the person has a 'need for water' can be tentatively made if (1) he has been kept from drinking for 24 hours, or (2) if, having had an opportunity to see that the left-hand path leads to water, he takes the left-hand path. Neither of these criteria is conclusive, and the second one would have to be repeated several times in order to be even highly indicative, but there can scarcely be any doubt that both of them are concrete and non-intuitive. Similarly, the existence of 'knowledge of *B* as a path to water' can be tentatively inferred: (1) if the person has often had an opportunity to see the water at the end of path *B*, or (2) if, having been deprived of water, he consistently enters path *B*. Here also the concrete, non-intuitive character of the criteria is apparent.<sup>17</sup>

It should be especially noticed that, as long as there are even two operational criteria for each of the crucial concepts, the logic of prediction can be non-circular. The concrete facts used as a basis for prediction are not, in this case, the same as the concrete behavior which is predicted. In the case of the need for water, we can tentatively infer it on the basis of the 24-hour water-deprivation, and we can check our inference by noticing whether the person takes the path which, in his previous observation, has led to water. Similarly in

<sup>16</sup> The writer has also prepared one complete deduction which will be gladly sent to any person who still suspects that there are 'unacknowledged gaps' in the Tolman-Lewin logic.

<sup>17</sup> It may be noted in passing that none of the four criteria depends upon introspection or even upon verbal report. It would of course be a useful supplementary verification of the other criteria if the person in the experiment told us that he was thirsty, or that he remembered the water at the end of path *B*. Such a report is not, however, an essential part of the logic.



the case of knowledge of *B* as a path to water: we can tentatively infer that the organism possesses such knowledge if we know that it has often seen water at the end of path *B* (under conditions favorable to this type of perceptual organization), and we can check the inference by noticing whether, when deprived of water, the organism enters path *B*. Where we actually get converging lines of evidence of this sort—where our hypotheses are overdetermined by our data—there need be no question whatever as to the methodological legitimacy of the two constructs, 'need' and 'knowledge.'

#### IV. ECONOMY

1. *Greater predictive power with the same number of postulates.*—On the score of economy the S-R psychologist sometimes argues as follows: "Yes, I'll grant that your postulates apply fairly well to certain unusual forms of learning, such as 'latent learning'; but the fundamental phenomena of learning are more simply accounted for by my postulates than by yours. For instance, the commonest and most basic sort of learning is trial-and-error, which is covered by my 'law of effect' or 'principle of reinforcement.' You can use your more complex postulates to describe the more complex and atypical forms of learning; but I'll stick to reinforcement as the simplest, most economical explanation of the fundamental process. And perhaps, ultimately, I'll be able to account for your complex type of learning in terms of my simple one."

The S-R psychologist who argues in this way is overlooking one simple but absolutely fundamental fact: the Tolman-Lewin postulates cover adequately *both the phenomena of 'trial-and-error' or 'reinforcement' and the phenomena of latent learning*, while the principle of reinforcement, stated in S-R form, covers only the former. Moreover, the S-R prediction actually rests upon two basic assumptions, which is the same number that the Tolman-Lewin predictions rest upon. The Tolman-Lewin postulates therefore have greater predictive power with the same number of postulates, so that, if this ratio is our measure of economy, the Tolman-Lewin postulates are actually superior in this respect.

Let us examine once again our illustrative experiment. The S-R psychologist might say, "Yes, you can predict well enough the left-turn-when-thirsty. That is clearly a case of latent learning, because the learning is not manifest until the motive changes from hunger to thirst. But how about the right-turn-when-hungry—the more typical sort of learning which occurred before the motive was changed? The person went to the right by chance, he was rewarded, and he then continued to go to the right. Isn't that a clear case of ordinary reinforcement or trial-and-error learning?"

What such an interpretation overlooks is that Tolman or Lewin could predict the right-turn-when-hungry (and not merely label it) on exactly the same basis as that on which he would predict the left-turn-when-thirsty. The person has *perceived* food at the end of the right-hand path. He now wants food. He will therefore probably take the right-hand path. This reasoning is in principle exactly the same, and can be made more completely rigorous in exactly the same way, as the reasoning we have already considered as a basis for predicting the left-turn-when-thirsty.

In other words, there are not two sorts of learning, 'reinforcement' and 'latent learning.' There is essentially only one type—perceptual learning. In both cases this is the only sort of learning that needs to be assumed. 'Latent learning,' which the S-R psychologist often assumes to be a particularly complex phenomenon, thus turns out to be no more complex than the sort of learning involved in ordinary trial-and-error. In fact, the two sorts are identical. In both cases the person simply perceives, and later remembers, a spatial relationship. Whatever influences the adequacy of his perception will also influence, to the same degree, the adequacy of his learning.

The factor which makes it appear at first that there have been two sorts of learning is, of course, the factor of motivation, and the fact that the motive was changed in the course of the experiment. As long as the motive remains what it was at the time of the original perceptions (*e.g.*, when our hungry person remains hungry), the logical outcome of the Tolman-Lewin postulates is behavior which is identical with

the behavior which S-R psychologists attribute to 'reinforcement'—*i.e.*, the sort of behavior which has been followed by a reward situation. When, however, the motive changes, exactly the same postulates lead to a very different prediction. The type of learning does not change; it is perceptual learning in both cases. The only variable which has changed is the motive; and this change occurs *after* the learning has occurred, so that it could not be the determining factor as to what 'sort' of learning has occurred.

It is of course unfair to judge the predictive power of S-R psychology by only one of the postulates which have been formulated in S-R terms. It is equally unfair, however, to judge the predictive power of the Tolman-Lewin approach by only two of the postulates formulated in their terms. Without going further into detail it can be said that, speaking very roughly, the two interpretations make equally adequate predictions in all those cases of learning where the motive at the time of performance happens to be the same as the motive at the time of learning, while the Tolman-Lewin interpretation makes superior predictions whenever the motive at the time of performance is different from the motive at the time of learning.

And, actually, it does so with an equal number of basic postulates, because the S-R interpretation also rests upon two postulates rather than one. It contains an implicit postulate which often goes unstated—the assumption that behavior, at the moment when it occurs, is determined by certain entities called S-R connections, which directly connect the perception of an external situation ( $S_1$ ) with an overt response, or which directly connect a drive stimulus ( $S_D$ ) with an overt response. It is only when this assumption has been made that 'the reinforcement of an S-R connection by a reward situation' can have any real meaning or lead to any definite predictions about behavior. The S-R connection is, in fact, a theoretical construct—an operationally definable and hence a legitimate construct, but nevertheless a theoretical construct, on the same methodological level as the 'needs' and 'knowledge' which are postulated by Tolman and Lewin. To say that

behavior is directly determined by an additive (or multiplicative) combination of S-R connections is to make a postulate, on exactly the same methodological level as our path-goal postulate which states how needs and knowledge must be patterned in order to produce a given piece of behavior. This, postulate, and the reinforcement postulate, constitute the basic theoretical outlay of Thorndike, Hull, and the other S-R psychologists who stress trial-and-error learning. But Tolman and Lewin, with an equal outlay, can make a larger number of valid predictions.

2. *The restricted law of effect may be deduced from perceptual learning and path-goal behavior.*—To put it differently, the 'law of effect' is not contradicted by Tolman and Lewin, but can be incorporated as a theorem in a field-theoretical system. Or rather, a new 'law,' covering the actual facts which have been attributed to an S-R 'law of effect,' can be deduced from the perceptual-learning and path-goal postulates. As Carr has pointed out (3), it is necessary to distinguish between what he calls the 'empirical law of effect' and the S-R interpretation of the behavioral facts. As he expresses it, the 'empirical law of effect' merely states that responses followed by reward are more likely to be repeated than non-rewarded responses are. As Tolman pointed out in the same symposium, however, even Carr's empirical law of effect covers more than the experimental data actually warrant (26). The generalization is empirically justified only if it is stated with the proviso that *the same motive must continue to exist*. It might then be called the 'restricted empirical law of effect,' or simply 'the restricted law of effect.' It could be stated formally as follows: If a particular piece of behavior is soon followed by the satisfaction of a particular motive, there will ordinarily be an increased tendency—when the same situation is again presented and when the same motive again exists—for the same behavior to occur again.

The reader will probably see immediately the essential relationship between this empirical generalization and the two basic postulates that have been discussed here.<sup>13</sup> If this rela-

<sup>13</sup> A formal deduction will be sent on request.



tionship is accepted, it follows that the assumption of a special mechanism for 'reinforcement' is an unnecessary, *uneconomical* hypothesis. If we have already accepted the perceptual-learning and path-goal postulates (as we must, in order to account for such facts as the left-turn-when-thirsty), then it is unnecessary and uneconomical to assume also a special mechanism for reinforcement.

## V. EXPERIMENTAL BASIS

1. *Experiments on the human level.*—The S-R psychologist might have one final argument. He might say: "You talk about 'common sense,' but science and naive common sense are two different things. The test of a scientific hypothesis is not whether it agrees with tacit everyday assumptions, but whether it leads by rigorous logic to predictions which are then verified by well-controlled experimentation. How can you explain away the very large mass of careful experimentation conducted in the laboratories of S-R psychologists?"

Let it be granted immediately that agreement with naive common sense can be considered only a very tentative basis for favoring any hypothesis. Experimental science has repeatedly disproved naive popular assumptions (*e.g.*, Galileo's experiment on the speed of falling bodies); and in such cases, where there is actual conflict, the well-controlled experimental data should unquestionably take precedence over uncontrolled observation or snap judgment. It may be noted that 'agreement with common sense' was not one of our four criteria for a scientific set of postulates. Neither originality (which S-R psychology possesses) nor an unoriginal agreement with common sense (which S-R psychology does not possess) was included in that list. Both are unimportant from the standpoint of experimental science. Let us simply look, then, at the existing experimental data. Are these data in agreement with common sense, or in conflict with it?

The data can be divided immediately into the human and the infra-human. As Hull has suggested, it is quite conceivable (apart from the experimental data) that what we have called perceptual learning and path-goal behavior are charac-

teristic only of human organisms possessing language, and that non-linguistic organisms do not show the same mechanisms. The hypothetical S-R mechanism of reinforcement is simpler than the Tolman-Lewin mechanisms (even though it involves the same number of postulates), and it might be that the simpler organisms show it relatively more, and the Tolman-Lewin mechanisms relatively less, than the most complex organisms do. At least the possibility is worth investigating.

About the human data there is perhaps not a great deal of argument. It seems possible that, if the Tolman-Lewin postulates are stated in the simple and modest form which has here been suggested, there are actually very few present-day psychologists who would not be willing to accept them, or their terminological equivalents.<sup>19</sup>

On this limited issue of whether human subjects show a tendency to perceptual learning and path-goal behavior, there is an abundance of supporting evidence. There is, for instance, the experiment which we have been using as an illustration throughout this article. As was pointed out, this experiment (originally devised by Spence), is as near to being crucial as any experiment is likely to be. It has not been carried out on the human level in exactly the form described here. That description was simplified for purposes of concise exposition. The writer has carried out informally, however, an experiment which would seem to be analogous to it, in all essentials. It is an extremely easy experiment, which the reader can easily repeat if he has any doubt whatever as to its outcome.

To make a person repeatedly hungry and then thirsty involves an unnecessary consumption of time, and unnecessary discomfort. They are unnecessary, because human motives can be changed—if the subject is willing—by the simple device of varying verbal instructions. The experiment was therefore carried out in a modified form, as follows: The experi-

<sup>19</sup> It may be noticed that, as here stated, the postulates lead only to the prediction of a 'tendency' to certain kinds of behavior, and do not assert that this tendency is the only one operating. They do not even necessarily exclude an S-R reinforcement mechanism, as a concomitant mechanism, of minor importance, which sometimes supports and sometimes counteracts the mechanisms of perceptual learning and path-goal behavior. Some of Thorndike's data suggest, indeed, that this may be the case.

menter holds a coin in each hand, a nickel in his right hand and a penny in his left hand. Opening both hands, he asks the subject to look at them carefully. He then closes them and says "Now show me which hand has the nickel in it." The subject points to his right hand; he opens it and says enthusiastically, "Good!" This is repeated three times, and each time the response of pointing to his right hand is rewarded by saying "Good!" He then changes the motive of the subject—leaving the total situation (apart from the new motive) almost exactly the same—and says "Now show me which hand has the penny in it." The question then is: Will the subject exhibit the only response which he has ever practiced or which has ever been rewarded (*i.e.*, pointing to the right hand); or will he exhibit the behavior (*i.e.*, pointing to the left hand) which is seen by him as a 'path to' his newly acquired goal—the goal of showing his knowledge of where the penny is?

Admittedly, this is a *reductio ad absurdum* of the law of frequency and the law of effect. As was fully expected, all of the twelve subjects used in the experiment—twelve out of twelve—pointed to the experimenter's left hand. In fact, nearly all of them appeared to be amazed at the experimenter's naiveté in imagining that they might do anything else. Nevertheless, the experiment appears to be just as crucial as the experiment described in this paper.<sup>20</sup>

Much other experimental evidence on the human level is summarized in Woodworth's *Experimental psychology* (31), especially in the chapters on conditioning, maze-learning, and

<sup>20</sup> It might be noted that the S-R psychologist loses his logical consistency if he replies that "the situation changes completely when the word 'penny' is substituted for the word 'nickel' in the instructions to the subject." It is true that the motive changes, but all other components of the total situation remain essentially the same. The visual situation is not altered, and all of the words in the directive sentence, except one, remain the same. Any additive combination of S-R connections involving various components of the total situation should therefore lead (if the S-R psychologist is logically consistent) to a repetition of the former behavior associated with these unchanged components.

It may also be noted that the response of pointing to the experimenter's left hand (which actually occurs) could not possibly have been 'conditioned to,' or directly associated with, the word 'penny.' The task of accounting for the results in terms of either a conditioned-response or a law-of-effect psychology is, therefore, much more difficult than it might at first appear.

problem-solving. The careful reader will notice that Woodworth's critical and balanced interpretation, based on these data, is essentially the same as that of Tolman and Lewin. His hypothesis of 'place learning' is clearly a special case of what we have called 'perceptual learning'; and his statement on perception itself in relation to learning is worth quoting: "The animal psychologists have usually assumed movement to be simpler than perception. Judging by ourselves we should say that nothing is easier than seeing an object and that no learned reaction is simpler than recognition" (31, p. 124).

As for specific experiments, we can only pick more or less at random from a very large number. We will mention here just three experiments, chosen from three areas of experimentation which are the recognized strongholds of S-R psychology: conditioning, maze-learning, and rote-memory. In the field of conditioning there is Gibson's experiment (6) on hand-withdrawal, which indicates unmistakably that the response which is conditioned is in this case a piece of 'knowledge' or an 'expectation' rather than any overt movement. In the field of maze-learning on the human level there is Perrin's experiment (20), which he himself summarizes as follows: "The reports show that without exception the net result from the first trial was a *knowledge* of the general spatial relations" (italics ours). And, in the field of rote-memory, there is the very recent experiment of Wallach and Henle (29) on the law of effect, in which 'reinforcement' was found to be of no appreciable importance as compared with conditions determining what kind of perception would predominate.<sup>21</sup>

<sup>21</sup> It should be noted that superior validity and economy are here claimed for the Tolman-Lewin interpretation only in relation to the specific S-R principle of 'reinforcement,' interpreted as synonymous with Thorndike's law of effect. It is *not* claimed that the S-R formula is without value, as a first approximation, in dealing with other problems of conditioning, rote-memory, etc. (See above, pp. 164-165.) For instance, it is not claimed that Tolman or Lewin has anything simpler or better to substitute for Hull's 'mathematico-deductive theory of rote-learning' (14). Wherever the factor of motivation is held substantially constant at the time of recall (as it is in the typical rote-memory experiment, in which the subject is motivated or 'set' to express overtly the first syllable that 'comes into his mind' after seeing the stimulus-word), and where perceptual factors other than temporal contiguity are held substantially constant at the time of learning (as in the typical rote-memory experiment), the explanatory con-



2. *Experiments on the infra-human level.*—If instead of human subjects we consider rats, there is more real disagreement between different experimenters. Hull, for instance, while frankly granting the existence and importance of perceptual learning in man (though he is not satisfied with the rigor of the Tolman-Lewin formulations of it), places much more emphasis on S-R reinforcement than on perceptual learning in rats. In the original Spence-Lippitt experiment (planned by Spence essentially as we have described it on pp. 159-160), Hull predicted that while human subjects would go to the left, rats would predominantly go to the right. On the other hand, Tolman (26), Leeper (16) and others have contended that even in rats perceptual learning is the primary learning mechanism.

Here again the reader may find an excellent summary of the data supporting the Tolman-Lewin interpretation in Woodworth's *Experimental psychology* (31; pp. 124-141, especially p. 136). Tolman also has summarized some of the essential data in comparatively simple and familiar words (26). Probably the most crucial experiments are those on 'latent learning.' Whether or not the word 'latent' is appropriate (see above, pp. 175-177), the phenomenon itself seems to have been very solidly established. At least eleven experiments have testified to its reality and importance (Lashley, 15; Blodgett, 1; Tolman and Honzik, 27; Haney, 7; Daub, 4; Leeper, 16; Buxton, 2; Spence and Lippitt, 22; Porter, 21; Zener, 33; and Wallace, 28). And, since 'latent' learning must be attributed to perceptual learning rather than to re-

cepts of the field-theorist must become very similar to those of the S-R psychologist, even though he may still insist on using a different set of words. For instance, the field-theorist may say "activation of the memory-trace  $A-B$  by the perception  $A$ , provided that the need  $N$  also exists," and the S-R psychologist may say "arousal of the ideational response  $B$ , when the stimulus  $A$  is presented, provided that the drive-stimulus  $S_D$  also exists." In either case the occurrence of the 'idea'  $A-B$ , instead of a competing idea  $A-C$ , requires explanation; and the assumption that  $A$  is somehow connected with  $B$  more closely than with  $C$  is an essential assumption in any quantitative handling of the problem. The writer has even been led to suspect that the comparative lack of interest in this problem of "what comes into the mind," on the part of Tolman and Lewin, is partly due to an unacknowledged realization that, if they went into the problem in more detail, they would be forced to use concepts uncomfortably similar to the S-R formula.

inforcement, while rewarded learning is ambiguous in this respect, these experiments at least demonstrate the existence and importance of perceptual learning on the rat level. Whether there is also a special mechanism for reinforcement does not seem, as yet, to have been conclusively proved or disproved.

## VI. SUMMARY

1. *The Tolman-Lewin interpretation of learning is operationally meaningful.*—Two of its postulates, which have experimental implications differing from those of the S-R principle of 'reinforcement,' can be stated (in condensed form) as follows:

The perceptual learning postulate: The acquisition of 'knowledge' occurs whenever perceptual conditions are favorable, whether or not there is subsequent reinforcement.

The path-goal postulate: If an organism has a 'need' for a goal, *G*, and 'knowledge' of some behavior as a path to *G*, that behavior will tend to occur.

2. *It is rigorous.*—The key terms, 'knowledge' and 'need,' are here operationally defined.

3. *It is economical.*—It involves the same number of postulates as does the S-R principle of reinforcement, and it makes possible a larger number of valid predictions.

4. *It is well established experimentally.*—On both the human and the infra-human levels, the existence of perceptual learning and path-goal behavior have been amply demonstrated.

## VII. CONCLUSION

It may well be that the numerous critics of Tolman and Lewin are at least partly correct in their most frequent criticism, which is that Tolman and Lewin have dressed up ordinary common sense in new and unnecessary terms. It is unfair, however, to make this criticism without at the same time recognizing exactly how the proponents of 'reinforcement' had departed from common sense, and without recognizing the wholesomeness of Tolman's and Lewin's emphasis upon the common-sense principles of perceptual learning and

path-goal behavior. These two principles ought to be the explicit and universally accepted foundation of all learning theory.

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## REINFORCEMENT IN TERMS OF ASSOCIATION

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### I. POSTULATES, THEOREM AND PROOF

It is one of the most obvious facts of behavior that reward (including escape from punishment) reinforces the responses leading to it. It is also one of the best illustrations of the dictum that the most obvious facts are often the most difficult to explain. For after over 40 years of research in animal and human learning the problem of *how* rewarded responses are reinforced is still unsolved. An encouraging sign, however, is the variety of fruitful hypotheses under active experimental investigation (10). Although the ideas suggested in this paper represent no radical departure, I hope they will meet the criterion of usefulness.

Rather than attempt a general theory of reinforcement, it seems more profitable, at least for the present writer, to select a specific learning situation and try to explain it by means of suitable hypotheses. If they lead to experimental verification in this instance they may, on the one hand, be applied to a wider range of situations and, on the other, be subjected to more rigorous logical procedures. The case I have selected is one which has already been subjected to theoretical analysis (26), namely, that of a hungry rat in a single-T maze. Let us assume that every time he chooses the right-hand alley he finds a bit of food at the end; if he chooses the left he finds no food but is simply removed.<sup>1</sup> Normally after a number of trials the rat shows a preference for the right-hand path, at first accompanied by hesitation and vacillatory behavior<sup>2</sup> at the choice point, later expressing itself in a prompt and de-

<sup>1</sup> The 'non-correction' method is chosen because it is simpler. The 'correction' method is complicated by the fact that on each trial, regardless of the rat's choice, the correct path is taken and the reward secured.

<sup>2</sup> Hereafter referred to as VTE ('vicarious trial-and-error') (15, 26).

pendable turn which may start even before the choice point is reached. Our problem is to explain this remarkable change of behavior.<sup>2</sup>

The explanation here proposed requires certain presuppositions, of which a minimum is listed below. Some of these postulates are simply restatements of 'laws of association,' given here largely to avoid misunderstanding. My terminology has been influenced by Robinson's (19) penetrating discussion. By 'stimulation' I mean any pattern of impulses arriving in the central nervous system. By 'response' I mean any neural pattern capable of being set off by a stimulation. Although the events described can be given no precise physiological meaning I assume that they take place primarily in the brain.

I. *When a stimulation occurs together in time with a response it acquires a tendency to arouse that response or some portion of it (law of contiguity).*

Corollary 1. *A portion of a response exerts a tendency to arouse the total response.*

II. *The associative tendency (from I) is stronger if the stimulation precedes than if it follows the response and is inversely related to the time interval between them (Robinson's [19] interpretation of the law of contiguity; temporal gradient of classical conditioning [10]).*

III. *The associative tendency increases with the number of times the stimulation is accompanied by the response (law of frequency).*

IV. *When a stimulation occurs together in time with two or more successive responses the associative tendency is strongest to the last response (Guthrie's [8] theory of finality).*

Postulate IV may also be considered a special case of the 'law of recency' but probably involves a different relationship to length of retention interval from the case of separate stimulations.

V. *When a stimulation occurs together in time with a re-*

<sup>2</sup>How remarkable it is must be felt by every psychologist who has trained two rats in opposite directions on a maze, then placed these little animals, distinguished only by the marks on their ears, in the same situation, only to see each swing in its own direction with unerring precision.

sponse other stimulations resembling the first acquire a tendency to arouse that response or some portion of it (law of assimilation [23]; stimulus equivalence [12]).

VI. If the response (from I) is prevented the stimulation may combine with other stimulations to evoke other compatible responses.

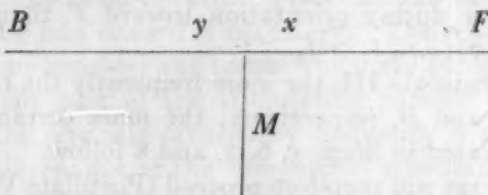
VII. *If stimulations to two incompatible responses occur together their effective excitation to those responses is diminished.*

VIII. *Of two or more response tendencies that one with the strongest instigation is the most likely to be completed.*

IX. *When a stimulation has acquired a tendency to arouse a response, the more intense the stimulation the greater the magnitude of the response.*<sup>4</sup>

Let us now return to the rat in the maze. Investigators have commonly recognized two stages in trial-and-error learning, *selection* and *fixation*. A third initial stage of orientation or exploration is usually presupposed. We shall consider first the process of selection. Following Hull's (11) excellent example I shall state the problem as a theorem and attempt a more or less formal proof.

*Theorem: If a hungry rat is repeatedly fed when he takes one path in a T-maze and removed without food when he takes the other, he will choose the path to food with increasing probability.*



Let  $M$  = any point in the maze,  $F$  = end of food path,  $B$  = end of blind alley,  $x$  and  $y$  = stimulation (e.g., visual, olfactory, proprioceptive) received when the rat points his

<sup>4</sup> Other factors undoubtedly play a part but are here omitted to avoid complication. A more complete list would include, for example, the assumption that a hungry organism is active and will respond to external stimulation by investigatory behavior. The propositions included are subject to debate which lies outside the scope of this article. Since they are very general statements which are still awaiting systematic experiments, it is impossible at present to define the conditions under which they hold. Particularly urgent is the question of the definition of needs as distinguished from non-motivational stimulations and of the extent to which the two enter into distinct relationships.

nose in the direction of  $F$  and  $B$ , respectively,  $S_D$  = hunger stimulation,  $\rightarrow$  = 'gives rise to.'

*Proof.*—1. Let us assume that the terms of the hypothesis are fulfilled. Then by Postulate V various features of the maze acquire a tendency to arouse a portion of the response to food. Let us refer to this portion as a food-surrogate,  $f$ . *I.e.*,  $M \rightarrow f$ .

2. In like manner,  $M \rightarrow b$ , a dead-end-surrogate.

3. But the impulses constituting the hunger drive have already, by Postulates III and IV, acquired a strong association with eating; *i.e.*,  $S_D \rightarrow f$ .

4. Therefore the food-surrogate is stronger than the dead-end surrogate (Postulate IX) and exerts a stronger tendency to complete itself (Corollary 1, I); *i.e.*,  $f > b$ .

5. At the choice point the rat pauses and makes tentative head movements to right and left (unexplained, but *cf.* Tolman [27]). When he points toward  $F$  he encounters stimuli which, by Postulate II, enhance the food-surrogate; *i.e.*,  $x \rightarrow f$ .

6. In like manner, when he points toward  $B$ ,  $y \rightarrow b$ .

7. But, by Postulate VII, this diminishes the instigative potency of stimulation active at the moment.

8. The available excitation, chiefly due to  $f$  (Step 4), is thus stronger during orientation toward  $F$  than toward  $B$  (Steps 5, 6, 7); *i.e.*,  $f_x > f_y$ .

9. By Postulate III, the more frequently the rat takes the paths to  $F$  and  $B$ , respectively, the more certainly will the results indicated in Steps 5, 6, 7, and 8 follow.

10. The rat will therefore proceed (Postulate VI) down the food path (Postulate VIII) with increasing probability (Step 9).

Q. E. D.

So much for selection. We have accounted for it essentially in terms of the differential strength of the food-surrogate produced by the alternatives at the choice point. Fixation, the final stage of unhesitating and even anticipatory choice, requires that the food-surrogate, as the chief excitatory tendency, become predominantly associated with the correct



turn. A number of principles which might be invoked to account for this—*e.g.*, backward conditioning, or a positive relation between strength of stimulation and strength of conditioning—suffer from lack of experimental support (10). But once the food path is selected in a large proportion of trials, fixation can be most readily derived from Postulate III, the much-maligned 'law of exercise.'<sup>5</sup>

In view of the criticism to which the principle of the 'law of exercise' has been subjected in recent years its use here requires some defense. Aside from attacks on the 'pathway hypothesis,' which need not be considered, the chief criticism is that the 'law' does not always hold. This can hardly be refuted. Repetition alone will not account for the acquisition or even the fixation of a new mode of adjustment. Repetition, however, never occurs alone, and the most confirmed opponents of 'repetition as such' admit the efficacy of repetition with reinforcement. For our present purpose we need look no farther than the experiment of Krechevsky and Honzik (13) for a convincing demonstration of the rôle of frequency in stereotyping a simple maze habit.

The argument here takes on a decidedly circular appearance. In attempting to explain the effect of reward I have invoked frequency, at the same time admitting that its effect is dependent on reward. The circle can be broken only by explaining *why* reward is necessary. The reason becomes clear when we consider the terms which are being connected. The dominant part of the stimulating pattern is the food-surrogate, the response favored by frequency is a right turn. But the maintenance of the 'prepotent' stimulation depends on the continued finding of food at the end of the path. The processes invoked to account for selection continue to operate during fixation. If the reward is removed the food-surrogate is blocked and the habit disrupted.<sup>6</sup>

<sup>5</sup> The elimination of VTE may also be deduced from Postulate IV.

<sup>6</sup> The importance of what I call the food-surrogate was impressed on me by a striking incident. A rat on an elevated single-T maze with 12-foot paths was apparently in a quandary, making false starts in either direction and behaving in a most disconcerted manner. Suddenly, with clearly visible mouth and tongue movements, he 'smacked his lips' loudly twice, turned and dashed directly down the correct path to food.

## II. DEDUCTIONS

The present account of maze discrimination hinges on the concept of the food-surrogate and its counterpart representing the dead-end, for it is the arousal of these competing patterns at the choice point which produces an increment of instigation in favor of the correct path. To prove its worth we must submit the hypothesis to the crucial test: Is it capable of yielding verifiable deductions?

In general it follows that any condition which increases the difference between  $f_z$  and  $f_v$  (see 'Proof,' Step 8) will increase the probability of a correct choice. Such a condition may affect both surrogates together or it may vary the strength of one independently of the other. In particular the following specific deductions are suggested:

1. If, in the early stages of learning, the animal delays at the choice point he is more likely to choose the path to food. This follows because the delay permits the successive arousal of anticipatory reactions. There is already a body of evidence supporting the deduction, for which we are indebted to Muenzinger and Tolman and their collaborators. They compiled data from a number of discrimination problems showing a positive relationship between VTE and performance (15, 26). They attributed this to the favorable effect of VTE partly on differentiation of the critical stimuli, partly on the 'reinstatement of after-effects.' The latter function is the one advocated here. In addition Muenzinger and Fletcher (17) showed that an enforced delay at the choice point accelerated the learning of a black-white discrimination. In the T-maze some rats, instead of VTE, form a position habit which they execute time after time without delay. It would be interesting to see if an enforced delay would break the habit.

2. Rate of learning should vary with the amount of difference between the end boxes of the maze.<sup>7</sup> If they are closely similar we may expect, by Postulate V, a high degree of generalization from food box to dead-end-surrogate, such

<sup>7</sup> It is assumed throughout that the end boxes are not visible at the choice point.

that the latter will also tend to evoke an eating response. The excitatory differential between the two paths is thus reduced. On the other hand, making the end boxes different—*e.g.*, in shape, size, color, or flooring—should improve performance.

Both of the foregoing deductions directly involve the relationship between the surrogates for goal and blind. The remaining conjectures deal with situations which may affect the two factors separately. Rate of learning should vary with any condition which varies the strength of either surrogate at the choice point. The food-surrogate lends itself to such manipulation in several ways.

3. The hungrier the animal the greater will be the probability of a correct choice. Since, by Postulates III and IV, hunger has a strong tendency to excite responses connected with food, by Postulate IX the greater the hunger the more the food-surrogate will predominate in determining the animal's choice of routes.<sup>8</sup> The effect of degree of hunger on performance in a multiple-T maze was amply demonstrated by Tolman and Honzik (28). At the same time a limiting circumstance may be found in the first deduction. If the rat is too hungry to pause at the choice point for VTE his performance may be impaired. Muenzinger and Fletcher (16) found evidence to support this suggestion.

4. A positive relation is to be expected between maze discrimination and amount of reward. A large reward, by Postulate IX, arouses more vigorous and prolonged eating than a small one. The food-surrogate will be correspondingly stronger and will increase the likelihood of a correct choice. Grindley (7) found improvement in the maze performance of chicks with increase of reward, and Wolfe and Kaplon (35) showed that this relation depended not merely on the size of the reward but on the number of pecks necessary to consume it. As an extreme case may be mentioned the effects of introduction and removal of reward on the maze performance of rats (3, 4, 29).

<sup>8</sup> The reasoning here could be made more cogent by showing, as I believe could be done, that strengthening the food-surrogate necessarily strengthens the incompatible dead-end response.

5. A third way of enhancing the food-surrogate in the case of spaced trials is by pre-feeding. A rat which has just consumed a little food in the starting box will presumably reach the choice point under the influence of a stronger eating tendency than one which has not. This may constitute the explanation of the bi-directional gradient of error-elimination discovered by Muenzinger, Dove, and Bernstone (18) in their endless maze. These investigators found the multiple-choice points just after as well as just before the food boxes better learned than those in the middle.<sup>9</sup> Bruce (5) ran thirsty rats to water and hungry rats to food in a multiple-T maze. He found that varying amounts of water given the thirsty rats just before a daily run tended to improve performance, but in the other group pre-feeding had little if any effect. It is not clear from the report that the food given in the entrance box was the same as that found at the goal; if not, this might have been a factor in the negative results. Anderson (2), on the other hand, found that pre-feeding for five seconds in a separate box improved the performance of hungry rats on a multiple-T maze in which they *never received a food reward*. Although Anderson (1) derived this result from the theory of drive-externalization, the present theory would predict it only in case food was received in the goal box. His results can be reconciled with this theory only if it be supposed that some degree of generalization occurred from feeding box to goal box.

Besides these methods of varying the strength of the food-surrogate, there are others which involve the structure of the maze itself and may be applied to either path or both. In view of the important part played in the present theory by Postulate II, we may suppose that any arrangement by which choice-point stimulations may be brought into closer temporal proximity with end-box responses will change the level of performance.

6. If external features of the goal box (e.g., color of walls,

<sup>9</sup> That it was not merely a serial position effect was demonstrated by their 'start-to-goal' group, which, with one trial a day, showed a drop in errors before the goal but none at the start.



type of flooring) are also present at or just before the choice point, performance will presumably improve. For these features, being closely associated in time with eating, should evoke a stronger food-surrogate than otherwise at the choice point. A similar result is to be expected if stimuli encountered at the end of the blind alley are introduced at the choice point as well, since these, by increasing the dead-end surrogate, should lower the excitatory tendency to enter the blind.

7. If, as in our original example, a right-hand turn leads to food, then if a second right turn is inserted in the correct path just before the food box we may expect improved performance. Conversely, if a left turn is introduced at the same place performance should be impaired. Similarly, a left turn just before the end of the blind alley should make for more correct choices, while a right turn there should result in less. These deductions involve the assumption that among the stimulations determining a choice are proprioceptive impulses from the initiation of a right or left turn. If these impulses are duplicated just before food is reached, when again aroused at the choice point they will, by Postulate V, enhance the excitatory food-surrogate and facilitate their own completion. Performance will be improved or impaired depending on which turn is thus emphasized. In the same way a turning movement duplicated just before the end of the blind alley will provide increased proprioceptive instigation of the dead-end surrogate at the choice point. The result of this duplication, by Postulate VII, will be to depress the food-surrogate and with it the chances of completing the turn. Although the deduction awaits confirmation, Miller (14) presented evidence that a turning movement made in securing food may influence the rat's subsequent approach to the goal.

8. If a delay is introduced between making a choice and reaching either end box, provided that food is not stimulating the animal during the delay, the percentage of correct choices should be lowered. Moreover, within limits the longer the delay the greater the decrease to be expected. This deduction follows from Postulate II. Hamilton (9), using a Warden multiple-Y maze, found that a one-minute delay

after the last choice increased trials and errors to learn, but that further delays up to seven minutes were reflected only in time scores. Wolfe (34) used the non-correction method with a single-T maze and found a negatively accelerated drop in the percentage of correct choices with delays up to 20 minutes; the greatest part of the drop, however, fell within the first 30 seconds.

9. Another obvious way of changing the temporal relations of choice point and end box is by varying the length of the correct path or the blind alley or both. If this is done, using the non-correction method, we may predict an inverse relation between length of either path and rate of learning. An experiment to test this prediction is now in progress. If the correction method is used the deduction becomes more complicated. It still holds for variation in the length of the correct path alone. When the blind alley is shortened, however, two opposed results may follow. On the one hand the blind entrance is more closely associated with its end, increasing the difference between the alternative stimulations at the choice point; on the other hand the blind entrance is brought closer in time to the attainment of food, resulting in a greater possibility of confusion between these complexes. In an experiment using the correction method and changing the paths of an elevated single-T maze from twelve to three feet either separately or together, I found a marked improvement when the correct path was shortened but no reliable change upon shortening the blind (21).<sup>10</sup> Tolman, Honzik, and Robinson (30) used a three-choice situation involving two blind alleys, a long one and a short one. Their hungry rats entered the long blind less often than the short; for their less hungry rats the reverse was true. According to the above analysis the preference of the hungry rats for the short-blind entrance would be attributed to its greater temporal proximity to food; this effect may have outweighed its relative closeness to a dead-end. In rats under the influence of a weaker food-surrogate the dead-end effect was apparently the stronger. It would be interesting to see if similar results

<sup>10</sup> Excluding the results of three rats which failed to learn the longest maze.

could be obtained by varying the length of the blind in a two-choice maze.

As regards certain of the above deductions (1, 3, 4, 5, 8, 9) there is already experimental evidence, some favorable, some equivocal. As to the others (2, 6, 7) no evidence, so far as I am aware, exists at present. These should therefore be considered most significant for the theory.<sup>11</sup>

### III. RELATION OF THE THEORY TO OTHER VIEWS

The final section will be devoted to the relation of the theory here outlined to other views on the nature of reinforcement. Hilgard and Marquis (10) have provided the standard 'frame of reference' for the problem by means of the three principles of *substitution*, *effect*, and *expectancy*. Where does the present account belong with reference to these criteria? Since it is frankly couched in terms of association it seems most closely related to the first. But the answer is not so simple as that. For actually all three principles involve, explicitly or implicitly, a concept of association. *Substitution* relies on it exclusively. *Effect* postulates connections strengthened in accordance with their nearness to satisfying states of affairs. *Expectancy* presupposes some mechanism of association whereby a present situation can evoke an anticipatory reaction.

Let us consider these three categories in more detail. *Expectancy* is not a simple principle. Besides the mechanism above referred to it involves a demand for or against the anticipated consequent. But how does a consequent obtain its demand value? Tolman (25) asserts that the ultimate goals of behavior are innately provided. They consist, in the case of appetites, of 'certain quiescences to-be-reached' and, in the case of aversions, of 'certain disturbances to-be-avoided' (p. 271). It is difficult to conceive of quiescence or absence of disturbance as a response, however broadly we define the latter. It is equally difficult, therefore, to see

<sup>11</sup> In carrying out such experiments the difficulty of the task must, of course, be adjusted (by varying length of path or delay of reward and distribution of trials) to a level which will permit the factor under investigation to reveal its effect.

how it can function in the organism as a goal. In the case of organic needs with well-marked consummatory responses, e.g., hunger-eating, it seems plausible, even necessary in the light of evidence, to suppose an innate readiness of the end response. Such a supposition would be enough in the example of the hungry rat here considered. But in the case of external sources of discomfort which lack any one method of relief the assumption becomes harder to justify. And if it fails in some instances we are naturally led to seek some further principle which may hold good for all demands.

The *law of effect* provides a ready answer. A satisfier, according to Thorndike (24), sets off a confirming reaction which intensifies the activity of temporally adjacent connections and thus increases the probability that they will operate again. The generality of this postulate has recently been refuted, however, by the ingenious experiments of Wallach and Henle (31, 32). In a verbal multiple-choice situation presented as an ESP test with no incentive to learn, responses called right were repeated no more frequently than those called wrong. It may well be that in experiments of this sort E's signals are chiefly informative rather than impressive; they are effective only in so far as they initiate further operations on the connections signalized.

Hull (11), too, gives the principle of *effect* a central position in his system. He has recently defined his 'reinforcing state of affairs' explicitly as 'a situation involving a present or past reduction in a need.'<sup>12</sup> He then makes the increment of an  $s \rightarrow r$  tendency a function of its proximity to such a reinforcing state. Thus at one stroke he provides for the attachment of the drive stimulus to the goal response and for the selection of those ancillary responses leading most directly to the goal. But it is this very unification which seems to be the chief weakness of Hull's formulation. For it makes it impossible to separate the influence of reinforcement on learning from that on performance. If need-reduction actually strengthens  $s \rightarrow r$  connections according to their nearness it is difficult to explain the abruptness of maze-performance

<sup>12</sup> *Basic behavioral concepts and postulates*, memorandum of July 8, 1941.



shifts after introduction, removal, or changes in kind of reward (25). These results are more easily explained by considering the reward merely as one, though a most important one, of the associated factors in the situation determining the choice of response.

White's 'completion hypothesis' of reinforcement may also be considered a version of the *law of effect*. He proposed that "the completion of a fractional anticipatory reaction tends to reinforce recent and concomitant S-R connections" (33, p. 399). When, however, in order to account for latent learning, he extended the principle to include the fulfillment of expectations, he left it in a vulnerable position. As Stephens (22) recently pointed out in criticizing *expectancy*, reinforcement depends much more on the desirability of an outcome than on whether or not it is expected.

Can this very importance of reward or desirability as a behavior determinant be explained without resorting to innate goals on the one hand or habit-strengthening on the other? Guthrie (8) says yes. According to what I have referred to as his 'finality theory' of reinforcement, a persistent stimulus, drive, or annoyer is associated with the response which removes it. All other responses are dissociated from it, each by the one which follows. This form of the substitution principle would account for the demand of the hungry animal for eating as well as that of the suffering animal for the activity which has brought relief. What of its validity? So far the hypothesis remains without crucial experimental support. In an experiment designed to test it (20) I found that hungry rats learned to press a bar more readily when rewarded by food than when merely removed from the situation. The specific consequence of the act, over and above its finality, appeared to determine whether or not it was performed. This finding brings us around to *expectancy*, with differing demand values, and the problem of how to account for them. Since this factor was uncontrolled the test was not crucial; it should be possible, however, to devise more critical experiments.

A choice between *substitution* and *effect* is still premature,

yet the former seems at present in the stronger position. More experiments are necessary, such as that of Finan and Taylor (6), in which the effect of motivational factors on habit strength is separated from that on habit utilization. Maze discrimination should yield evidence to assist us in this choice. If reward strengthens near-by associations, then varying the length of time in the food path should prove more effective than similar variation in the blind alley. Meanwhile the burden of proof is on those who claim for reward a unique effect on learning.

This brief review may serve the twofold purpose of 'placing' the writer's theory and of pointing out some of the arguments which have dictated its present form. In some respects my own view most closely resembles that of Tolman. The arousal of food- and dead-end-surrogates by choice-point stimulations is similar to his concept of sign-Gestalt. But whereas Tolman translates his sign-Gestalts into appropriate action through the medium of demand value, I have attempted to account for the process by means of the relative strengths of competing instigations. And whereas Tolman derives the demand value of food from an innate demand for quiescence, I base it on the frequency, in the life of the organism, with which eating has removed the stimulation of hunger.

In some respects the present theory is patterned after Hull's. The notion of a food-surrogate is analogous to the 'fractional anticipatory goal reaction' which plays such an important part in Hull's system. Moreover, the determination of a choice by the combination of excitatory tendencies is characteristic of his theory. But Hull sets up a gradient of reinforcement-delay from which he derives both food-demand and the strengths of component habits. The present theory separates food-demand from what Tolman calls learning "what comes after what." The gradient by which it accounts for the latter is the 'temporal gradient of contiguity' (Postulate II). By keeping these two factors separate it is possible to avoid Hull's difficulty with the facts of latent learning.<sup>13</sup>

<sup>13</sup> Hull has recently succeeded in deducing latent-learning phenomena from his own postulate system. (*Memorandum to Psychology 106, November 14, 1941.*)

As to the other factor, food-demand, the explanation here offered can be traced to Guthrie. The food-surrogate which mediates the demand is dependent on the close association previously formed between hunger and eating. This dependence in turn is deduced from Postulates III and IV. Postulate IV is essentially Guthrie's, although it departs from his 'all-or-none' concept of association and substitutes the more traditional view that associations are subject to increments and decrements of strength.

The answer to our original question, then, is that the present theory has borrowed heavily from proponents of all three concepts of reinforcement. Yet it has done so without departing from the framework of associationist doctrine. This fact in itself is an indication of the basic role of *substitution* in learning. In adhering to it as an explanatory principle I have ignored one other—that of innate demands—cheerfully, and rejected a third—the *law of effect*—with regret. Regret because the hypothesis has stimulated much valuable investigation and will continue to do so. But association theory is no less provocative. In addition it offers the possibility of a comprehensive formulation of learned behavior.

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# ON THE POSSIBILITY OF ADVANCING AND RETARDING THE MOTOR DEVELOPMENT OF INFANTS

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## I. INTRODUCTION

In order to determine whether or not the hypothesis to be discussed here is so obviously correct that it does not require proof, 30 psychologists and graduate students of psychology were asked to indicate their ideas concerning the possibility of advancing and retarding motor development. They were given the following instructions in mimeographed form:

Please indicate when, in your opinion, the groups of infants designated below will begin to walk alone. Do this by comparing each 'experimental' group with an unselected group of normal, healthy infants. Each experimental group is supposed to differ from unselected normal children only in the respect which is designated in the description of the group.

Experimental Groups	Estimated Average Onset of Walking		
1. Those muscularly weak	precocious	normal	retarded
2. Those unusually strong	precocious	normal	retarded
3. Those small for their age	precocious	normal	retarded
4. Those large for their age	precocious	normal	retarded
5. Those who have been ill	precocious	normal	retarded
6. Those unusually healthy	precocious	normal	retarded
7. The feeble-minded (as determined later)	precocious	normal	retarded
8. The gifted (as determined later)	precocious	normal	retarded
9. Those greatly restricted	precocious	normal	retarded
10. Those given intensive training	precocious	normal	retarded

All but two of the 30 persons questioned thought that some of the experimental groups would walk earlier than the comparison group of unselected children. The groups most favored as probably walking early were those given intensive training and those intellectually gifted. Two-thirds of the

respondents indicated that they believed that infants given intensive training would walk precociously and almost as many felt that the unusually strong and the intellectually gifted would prove, on the whole, to be early walkers.

In view of these results, it appears that the theory which we wish to present below is not self-evident. Indeed, it will probably arouse considerable opposition. The theory to be presented maintains that infants who are superior to the average child in some one respect, such as in strength or in intelligence or in training, will not be advanced in motor development. On the other hand, children suffering from a corresponding deficiency, such as weakness, dullness, or restraint of practice, may nevertheless be expected to be retarded in motor development. In other words, it is proposed that a factor, which when deficient will cause developmental retardation, will not, when present super-normally, cause any precocity. This apparent paradox can, we believe, be solved by a consideration of the interrelationships of factors essential to motor development.

The discussion which follows will be oriented primarily toward the act of walking; however, this act may be taken as typical of any complex coördination which is arrived at in infancy, and hence our remarks may be considered to be a general discussion of the motor development of the young child.<sup>1</sup> After stating our theoretical views we shall review certain facts concerning the onset of walking in order to determine how well they fit the theory. Walking is chosen as a test performance because more data are available relative to it than are available with regard to any other infant response.

Our theory consists of three postulates and two consequences which can be deduced therefrom. The postulates, we believe, will be accepted by all. They are, indeed, very old and very common presuppositions, and we lay no claim to originality in their enunciation. We believe, however, that the implications which we have drawn from these postulates are, in some respects, novel.

<sup>1</sup> In a previous publication (7) we proposed, very briefly, the theory which we wish to expand in some detail in the present connection.

The first postulate is that there are multiple requirements for the onset of walking (and for any complex performance). It is not necessary to the presentation of our argument that we be able to specify these requirements. Let us label them A B C D E . . . N. We may indicate, however, that the existence of multiple requirements is a reasonable assumption; in fact, we believe that no other hypothesis can be maintained.

In the nervous system alone there are numerous requirements; among the necessary structures are parts of the cortex, many subcortical centers, and many sensory and motor nerves. The vestibular apparatus is essential to normal walking. Indispensable also are certain foot structures, a certain range of body proportions, and doubtless many other factors as yet unnamed, including, of course, all the organs which are necessary for life, such as the lungs, the heart, and the liver. As noted above, for our present purpose it is not necessary that we be able to specify all of the tissues of the body which are essential to a given coördination.

The second postulate is that these requirements are met not simultaneously but at various times. This too seems to be a reasonable assumption, and one in line with all recent work on human and animal development. A simple example of this principle is found in the fact that the heart is capable of functioning earlier than the lungs, that the peripheral nerves function prior to the functioning of the cortex, that the muscles can contract before the bones to which they are attached are ossified, etc. This sequence should not be confused with the sequential patterning of behavior. We are discussing in this postulate not the sequential appearance of behavior but rather the non-simultaneity of appearance of those items which make behavior possible.

The third postulate is that these factors have a considerable degree of independence in regard to their development. That is to say, retardation or precocity in the development of one of these requirements need not affect the development of all of the others. It is true, for example, that a feeble-minded child whose cerebrum does not develop normally may, nevertheless, develop relatively normal bones, muscles, and periph-

eral nerves. Another instance of independence is seen in puberty præcox, which speeds up the rate of ossification, but leaves the development of the nervous system unaffected. Again, an abnormal development of the limbs, such as a club foot, need not imply any other abnormality. It is not claimed, however, that the factors involved in walking are absolutely independent.

From these postulates two deductions or implications may be drawn. They are as follows:

*Proposition I.*—The onset of walking can be delayed by postponing the fulfillment of any *single* essential factor to a date later than that at which walking would normally have occurred. If N, the last requirement to be met, ordinarily appears at thirteen months, then if any factor is postponed beyond that date, walking will be retarded. This is obvious, since walking cannot occur until all the requirements for walking have been met. Since there are many such factors, there are many causes of retardation in walking, and each one in itself is capable of retarding the onset of locomotion. It follows as a corollary that it ought to be relatively easy to induce retardation.

*Proposition II.*—The onset of walking can be induced at a specified precocious date only if *all* of the factors which are ordinarily achieved between the precocious date and the normal date of walking are moved forward in time to the precocious date. That is to say, if N which is ordinarily the last requirement to be fulfilled is moved forward to, let us say ten months, walking will not occur at that time. Several other factors must also be moved forward, if walking is to be precocious. This follows from the fact that walking cannot occur until all the necessary factors are present. Precocity in walking must ordinarily involve the combined precocity of several prerequisites. The greater the precocity in walking, the greater the number of factors which will have to be achieved prematurely. Consequently, it should be relatively difficult to induce walking at a date much earlier than the average date. Very important in this connection is the postulate that the factors are relatively independent. If this were not the



case, the advancement in one factor would bring about the advancement of all of them, and precocious walking would be much easier to achieve.

Our postulates have now led us to the conclusions which we announced in advance, namely, that we should be able to find several conditions which when applied to a group of children will retard the onset of walking while conditions which will cause a group of children to walk precociously should be rare, and should be in the nature of multiple conditions rather than isolated factors. We shall now survey the facts concerning the onset of walking to determine whether or not they are in accord with these deductions. A summarizing discussion will follow.

## II. OSSIFICATION AND STRENGTH

These two factors must be treated jointly, since no investigation has yet separated them.

No doubt a certain degree of osseous and muscular development of the legs and spinal column is prerequisite to the onset of erect locomotion. As yet, the exact nature of this prerequisite is undetermined. It is known, however, that rickets is associated with faulty ossification and probably also with muscular weakness. Rickets is associated with a delayed onset of walking. While no average walking age for infants suffering rickets is available, Gesell (9) found that 18 per cent of the rachitic children examined by him could not walk at 2 years, whereas only 2 per cent of the non-rachitic subjects failed to walk by that date. The testimony of all writers on the topic of rickets is of the same character. Hutchison and Murphy (13) report that in India the children of Brahmins, whose mothers keep purdah, and who remain indoors with their mothers, walk much later than the children of women who do not keep purdah. The Brahmin children suffer much more often from rickets. In the same direction is the evidence that childhood illness interferes with ossification, and the finding of S. Smith (28) that children who have been ill in the period preceding the onset of walking are, on the average, six weeks later in beginning to walk than are

children who have suffered no serious illness during this period. We conclude, therefore, that retarded ossification and muscular weakness are capable of delaying the onset of walking.

On the other hand, in agreement with our theory, precocious ossification and unusual strength do not induce early walking. We have recently examined the case reports of puberty præcox with reference to this question (7). Puberty præcox, whose greatest incidence is in the first two years of life, greatly accelerates ossification. In addition, strength and body size are supernormal. On the other hand, there is nothing to indicate that neural maturation is affected by puberty, whether puberty be normal or precocious. The question of interest is, therefore, do children exhibiting puberty præcox begin to walk in accordance with their ossification and their muscular development? The answer, obtained by tabulating the walking ages of 25 case reports of very early uncomplicated puberty præcox found in the literature, is quite clear: puberty præcox cases, even those who reach puberty during the first year of life, walk, on the average, at the usual age. Precocious ossification, and likewise unusual strength and size, do not accelerate the onset of walking.

Certain evidence presented by Peatman and Higgons (23, 24) might seem contrary to this conclusion. These authors studied the motor development of a total of 349 infants who received exceptionally good pediatric care, whose health records were excellent, and whose growth rate was above the standard norms. The authors report that the motor development of these children was superior to the norms of Gesell and Thompson (10). These comparisons are in terms of the percentages of the two groups sitting, standing, and walking at each of several ages. The numbers examined at any age are scarcely sufficient to yield reliable differences. Furthermore, Peatman and Higgons (24) call attention to possible genetic differences in the two populations and to differences in the criteria for 'walking alone' employed in the two investigations.

Within their own group of subjects, where procedure and criteria were uniform, Peatman and Higgons find that with age held constant neither weight nor body build bore any significant relation to the onset of standing or of walking alone. In so far as weight is an index of osseous and muscular development, it would appear that onset of walking is not affected by these factors.

Often cited is the tentative conclusion of Shirley (26, p. 126), that "In general it may be stated that thin, muscular babies and small-boned babies walk earlier than short, rotund babies and exceedingly heavy babies." It is unfortunate that this statement has been quoted frequently, since Shirley reported walking ages for only 21 children, a number quite inadequate to establish the relationship expressed above. Her report should be considered to have been superseded by that of Peatman and Higgons, who found no such relationship.

### III. INTELLIGENCE

The onset of walking is definitely delayed in those of low intelligence. The first person to report data on this problem was Ireland, who in 1900 (14) found that the average age at the onset of walking in 111 cases of feeble-minded children was 2.5 years. Mead (19) confirmed the retardation of the feeble-minded in the beginning of ambulation. Mead found the average walking age of 144 feeble-minded to be 25.08 months. More recently Ordahl (22), Kuenzel (16), Abt, Adler and Bartelme (1) and Murphy (20) have presented similar data. Abt, Adler and Bartelme (1) and Murphy (20) have found that the more severe the mental deficiency the greater the retardation in the onset of walking. Murphy, for instance, found that the average walking age of high grade imbeciles was 21.58 months, whereas that of idio-imbeciles was 32.17 months. The correlation between Stanford-Binet I.Q. and the age at walking for Murphy's feeble-minded group was  $-.23 \pm .04$ .

The reason why mental deficiency should retard walking is not definitely ascertained. While feeble-mindedness un-

doubtedly means a slow development of the cortex, it may also mean a slower maturation of sub-cortical structures, and even of ossification and of muscular structure. Several writers have commented on the general physical inferiority of the feeble-minded. We are not acquainted with any data on the physical status of the feeble-minded during the first two years, but in view of their later subnormal condition, it seems best not to conclude that it is necessarily the deficiency in intelligence *per se* which delays walking in mental defectives.

An interesting fact, attested by the data of Ordahl (22), Kuenzel (16) and Murphy (20), is the finding that cases of Mongolism walk much later than do non-Mongolian defectives of the same intelligence. The average walking age of Mongolian defectives is approximately three years. This would suggest that in these cases some developmental deficiency in addition to defective intelligence is exerting an influence upon walking.

The converse of the effect of low intelligence, namely, that unusually high intelligence is associated with the early onset of walking, is not established. The assertion that this relationship exists was made by Terman *et al* (29), but these investigators employed for comparison with their gifted subjects only the data for fifty children of New York reported by Mead. While the average for the California gifted children is lower than that found by Mead for New York children, Terman's average is actually higher than the averages subsequently reported by M. E. Smith *et al* (27) for Hawaiian children and S. Smith (28) for Seattle children, and is not reliably different from the average derived from the data of Hrdlicka (4). M. E. Smith *et al* (27) have previously called attention to the error of Terman's conclusion.

A negative correlation between age at onset of walking and intelligence has been presented by Abt, Adler, and Bartelme (1). Examination of their published data shows that the relationship is definitely curvilinear, the entire correlation being due to the cases below an I.Q. of 70. Above that I.Q., no reliable correlation is indicated.



## IV. SENSORY HANDICAPS

As long ago as 1828 Necker de Saussure (21, p. 14) stated that "children who are born blind have no idea of walking, because they have never seen others walk. They are obliged to be first raised up, then made to stand, and afterwards to move one foot after the other."

Dr. Kathryn E. Maxfield, who has done extensive work with blind subjects of pre-school age, writes: "The fact that blind babies are retarded in walking is almost a truism to anyone who has worked with many of them."<sup>2</sup> Maxfield and Fjeld (18) have been preparing a Social Maturity Scale for blind and partially seeing children, based upon 101 visually handicapped cases of pre-school age. The item 'Stands alone' has been placed at one year nine months for blind children and the item 'Walks about room unattended' at two years. These placements are, of course, much later than the placements of these items for normal children.

Also of interest in connection with motor development are individuals lacking both sight and hearing. Here we are referring only to those persons who lose these senses before walking has become established. If the loss occurs after the child has begun to walk, we do not know of any data which indicate that walking is abandoned. There are several reports of children whose distance senses were lost at a very early age, and in whom walking did not appear until special training had been instituted. This was the case with Thomas Stringer, described by Anna Gardner Fish, whose account was published by Wade (29). Thomas was born with normal senses but lost vision and hearing quite early through spinal meningitis. When brought to the Perkins Institution at four years and ten months of age he crept but could not walk. He learned to walk quite readily when taught. The same story is told of three blind-deaf children who were admitted to the Texas School for the Deaf (2). Addie Lee Pruitt, admitted at age eight years four months, could not walk but in a few months "she was a rapid and fearless walker, defying obstacles

<sup>2</sup> Personal communication, January 18, 1941.

and refusing all assistance." Fred Wyatt Mursell, who entered the school at nine years ten months of age, could not stand erect even when given assistance, but soon learned to do so, and apparently learned to walk. Another child, Edgar Korte, admitted to the institution at age eight, came to the school carried in arms and could not walk, but nine months later walked well. Tait, an early deaf-blind case reported by Hibbert (12), did not walk erect. Hibbert makes the comment that his parents had never attempted to teach him.

The evidence from the blind and from the deaf-blind indicate that sensory deficiencies may delay, or even indefinitely prevent, the onset of walking. By what means this failure to walk is brought about we do not know. The failure may be due to a lack of social stimulation or to a lack of learning opportunities. However, the effect of sensory defects may take place by some other means. If the child is kept in bed because of his blindness, his inability to walk may be due to weakness rather than to blindness. Or, blindness may affect his walking by making him more timid or more fearful or more dependent. It is also possible that injuries sustained in early attempts at walking when blind may act to inhibit further attempts.

There are no data to indicate that children with super-average senses are earlier in walking, nor has anyone suggested that such might be the case.

#### V. SPECIAL PRACTICE AND RESTRICTION OF PRACTICE

We shall attempt to show in conformity with our general hypothesis that severe restriction of practice will delay the onset of walking while, on the other hand, an unusual amount of practice will have little or no effect.

First, we will consider the question as to whether or not restriction of practice can delay the onset of walking. Our own study of Hopi subjects must be discussed in regard to this question (6). We compared Hopi children reared by use of the cradleboard with those not restrained in this fashion, and found no reliable difference between the two groups. We feel that from this result sweeping conclusions should not be

drawn. As we stated in our original paper, the child is on the cradleboard chiefly when he is sleeping, and no one, we suppose, would hold that the child learns to walk while asleep. In addition to being restrained while asleep the Hopi child is on the cradleboard during some of his waking hours, but not for many of them. Toward the end of the first year he is freed from the board each day for longer and longer periods. It will be seen, therefore, that while the cradleboard restrains the child to some extent, it does not provide a total restraint. A much more extensive deprivation of freedom of motion and of opportunities to practice might cause a retardation in the onset of walking.

In our study of two infants (5, 8) reared under conditions of restricted practice and of minimum social stimulation there were three responses in which the subjects received no practice, namely, visually directed reaching, sitting, and standing. These responses could not be performed immediately when the subjects were tested at an age beyond the usual upper extremes for the respective patterns. The condition of restricted practice was not maintained to an age at which the subjects would have been expected to walk, but we are inclined to think that an infant who had been kept off his feet until he was tested for walking could not walk immediately. Our subjects could not support their weight on their feet when tested for the first time at one year of age; if the infant cannot maintain the erect position without practice it does not seem likely that he can walk alone without some practice.

This conclusion is reinforced by the finding, noted earlier, that blind children, on the average, are retarded in walking, and the finding that some blind-deaf children do not walk until given special training. While it is possible to explain these facts on other grounds, they fit very well the hypothesis that complete, or almost complete, lack of practice will delay the onset of walking.

This latter conclusion implies that the onset of walking normally involves learning, *i.e.*, that a certain amount of learning is one of the prerequisites for walking. As apparent

evidence to the contrary we need to discuss two reports, one by Woodworth (31) and one by Kirkpatrick (15). These accounts deal with children who apparently walked at the first opportunity. Woodworth reported a child who 'showed tendencies to walk' at nine months of age, but who, upon a doctor's advice, was put in long dresses in order to discourage precocious walking. When two months later she was by accident put on the ground without a long dress she "rose on her limbs and walked upright and without fear; after that the long dress did not keep her from walking, she raised it and walked."

Kirkpatrick's case, reported in 1903, was a bright and normal girl who was not walking at 17 months. One day her father placed his detachable shirt-cuffs on the living room table. The child became interested, crawled to the table, pulled to standing, and put the cuffs on her wrists. To do this she had to stand alone. Then to her father's great surprise she *ran* across the room. She walked readily when wearing the cuffs, but for several days would not walk without them.

In our opinion these cases fail to show that learning is unnecessary for the onset of walking. The child described by Woodworth had already 'showed tendencies to walk' before she was placed in a long dress. This must mean she had taken some sort of steps. Furthermore, a long dress, although it would not permit walking alone would presumably permit the child to pull to standing and to take small steps within the confines of her skirts while holding the sides of the crib or other furniture. In regard to Kirkpatrick's case, we may say that while it is clear that the child had never walked alone previous to the occasion described above, it is possible that she had walked holding to the furniture. Kirkpatrick, although not explicit on this point, seems to be amazed only by the child's walking alone, not by her pulling to standing. Such actions must, of course, be counted as possible practice in walking. It must be kept in mind that, by definition, there can be no practice in *walking alone* prior to the first performance. The practice lies in walking when led or when holding to a support,



or even in earlier activities. As Brainard has said (3, p. 244), "Since much balancing practice is gained in sitting and in standing by holding objects, it is likely that children learn to walk without actually walking."

We will ask next whether or not an unusual amount of practice can appreciably advance the age of walking. No one has systematically given to a group of infants an unusual amount of training specifically designed to hasten the onset of erect locomotion. There have been individual case reports. Preyer's son was given a great deal of exercise in walking when led, yet he did not walk alone until week 66, which is later than the average child (25). Such a case, of course, proves nothing, since we do not know at what age the particular child would have walked alone without special training. McGraw's (17) trained subject, Johnny, was given a considerable amount of special training, although not specifically in walking, and this training did not cause him to take his earliest steps unassisted before his untrained twin brother, Jimmy. This case also is not decisive, since the training was not designed to facilitate the acquisition of the response under discussion, and since the twins were not identical.

Of some relevance in the present connection are the walking records of infants born prematurely. If we consider children of the same life-age (age since conception), then at any life-age children prematurely born have had a longer post-natal existence in which to practice motor activities than have normal-term children of the same life-age. Hess, Mohr and Bartelme (11) have shown that a group of 91 prematures first walked at a *post-natal* age which was between two and three months greater than the average walking age of their sibling controls. While the life-age cannot accurately be ascertained, it is likely that the prematures were, on the average, between two and three months premature. In other words, it would seem that the prematures walked at the same life-age as their siblings, despite 2-3 months additional post-natal life in which the practice of coordinations leading to walking might have occurred. This conclusion is not entirely certain because premature birth may affect children in other ways in addition

to lengthening the post-natal period. It may, perhaps, delay their ossification, or hinder other growth processes. Nevertheless, one can say that the evidence available does not lead one to believe that the beginning of independent erect locomotion can be hastened by unusual exercise and practice.

#### SUMMARY

It is proposed that the infant cannot walk until a number of conditions have been met. If this theory of multiple requirements is accepted, it can be shown that a retardation in the fulfillment of *any* of these conditions should retard the onset of walking, whereas the early fulfilling of one or more of the requirements should not advance the age of walking so long as one or more remain unfulfilled. If our analysis is correct, it ought to be difficult, if not impossible, to find conditions which cause a group of children to walk at an earlier average age than do normal healthy children and easy to find conditions which retard the onset of walking.

We have reviewed the literature on the age at which walking begins and we believe that these theoretical expectations are met. We have found a number of conditions each of which will delay the beginning of walking beyond the average age of onset for normal healthy children. On the other hand, the respective antitheses of these various conditions do not advance the onset of walking. Puberty *præcox*, which involves extraordinary ossification and muscular strength, does not lead to early walking. The present evidence shows that gifted children walk no earlier than do children of normal intelligence. While experiments on the effects of intensive training have not been directed specifically at affecting the onset of walking, closely related experiments cause one to doubt that walking could be appreciably advanced by excessive practice, because the absence of the necessary neurological maturation would render the training ineffective. The evidence conforms to our expectation that a response whose development involves many essential factors can be advanced only by a combined precocity in several of the factors.

It is proposed that the implications of multiple requirements which have been demonstrated in regard to walking also hold with regard to many other infant performances.

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## THE DEVELOPMENT OF PARANOIC THINKING

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Two of the commonest attacks upon the problem of paranoic thinking agree in making the same mistake: They confine their hunt for the sources of delusions to the isolated organism. One of these approaches is concerned almost entirely with the central nervous system, while the other limits itself to an equally restricted psyche. So far as paranoic thinking is concerned, several decades of searching in the nervous system on the part of competent investigators have yielded us practically nothing. Even in those cases where brain lesions can be demonstrated, there is nothing specific or consistent about their character and location. In the great majority of cases of delusion no changes that are relevant and consistently present have been demonstrated, either macroscopic or microscopic.

The other attempt to account for paranoic developments is still gaining converts; but it belongs just as definitely to Nineteenth Century thought. This attack begins by postulating a 'psyche,' present at birth, which is itself unreal and has as its great enterprise in life the task of contacting a world of reality. In this process it is supposed to acquire layers of psychic ectoplasm which serve the double function of insulating it from reality and at the same time of enabling it to operate upon the real world. On this rather weird foundation have been erected the most intricate superstructures of theory which have actually come to dominate psychotherapy in this country today. One consequence of this development is that practical workers in the abnormal field are forced to accept, as current and new, the old shopworn division of our universe into two different worlds, an unreal world that we know directly in our 'psyche' and the real world which can only be known through the unreal one.

This view is unnecessarily complicated and indirect; in terms of human living it makes hardly any sense at all. Nevertheless it is still important to the public because, like belief in the devil, it is so widespread; it is important to professional workers because its defenders have managed to make skepticism about these views reflect somehow upon the critic rather than upon the theory. So far as paranoic processes are concerned, there is one assumption imbedded in this double-world hypothesis that is especially relevant to our immediate problem. This unreal 'psyche' is supposed to start out from earliest infancy with a whole collection of ready-made attitudes, opinions and interpretations, many of them frankly delusional (5, 6), the derivation of which is left unexplained. With such equipment this lonely psychic ghost begins its journey of discovery and disillusionment through a coldly realistic world.

I propose to avoid the unnecessary difficulties of the first of these theories by looking for the conditions under which paranoic developments arise in the field of social behavior, where they are actually found, instead of in the nervous system which, although always deeply implicated in behavior, is still not its *locus*. To avoid the chief defects of the psychic theory, I shall simply refuse to divide the world into real and unreal; and I shall postulate no more regarding the attitudes, opinions and interpretations of neonatal and postnatal infants than my own approach demands. In what follows, the main emphasis will be upon the commonest form of paranoic thinking, in which misunderstandings and misinterpretations develop into delusions of persecution, and finally erupt in reactions of retaliation or defense.

Delusion is basically a disorder of interpretation; and the possibilities of misinterpretation are being continuously realized in every normal person from a very early age. In fact, it is only through continual interchange of attitude with other persons, and through the modifications and corrections interchange involves, that adequately socialized behavior is built up and maintained (1, 2). In the prelanguage child this process begins, of course, at a more general behavior level.

As the child acquires more and more language behavior, the machinery for the correction of his misunderstandings becomes more highly developed; but at the same time language creates greatly increased opportunities for other and more complex misunderstandings. In childhood and in adult life one quickly discovers, without venturing into the abnormal at all, that mistakes once acted upon or brooded over can generate cumulative misinterpretations. In this way even normal persons can build up fairly comprehensive systems of misrepresentation in a very little time. Sometimes it takes a great deal of explanation and auxiliary evidence before the damage can be undone. The farther such a system develops the farther it tends to get away from what is 'true' for others, that is, from what holds good from several perspectives and not just from one's own immediate perspective. Paranoic thinking always involves some serious defect in the ability to alter perspectives and, as a natural result, a strong tendency to accumulate progressive misunderstandings.

In making a study of any abnormal development it is essential not only to consider the whole organism's performance and the biology that makes it possible, but also to include the participation of the effective social environment. Paranoic attitudes and actions make their appearance in such an environment. They grow out of a breakdown in the machinery of social cooperation. The mutual sharing of plans, acts and consequences, that goes to make up genuine communication gets replaced gradually, in certain persons, by solitary behavior in which there is an irresistible selection of the evidence on the basis of their special sensitiveness.

By communication I mean here conjoint activity in which two or more persons share an object or an act in its context. Obviously, the behavior of the persons must also be included as part of such a situation, as well as the attitudes that each person has toward himself and toward the other one. Language, which in its broader sense includes signs, gestures, sounds and symbols, always plays a particularly important role in paranoic conduct. I shall maintain the position in what follows that communication, in the above sense, is

fundamental to the development of language in the child, and that both underlie the organization of thinking in adults. Regardless of whatever preconceptions one may have had concerning the nature of adult thinking, as soon as one gets involved in studying an actual paranoic situation it becomes acutely evident that one is knee-deep in a disorder of communication (3).

The attitude of a person toward himself plays an important role all through paranoic thinking. Very early in life every child begins to develop habits of reacting to his own appearance and behavior in much the same way as others react to him. Worked into the organization of such habits are the reactions, attitudes and opinions others have regarding him. In this way his own repertory includes distillates of their responses to him; and so he comes to pass judgment upon his own conduct as he passes judgment on the conduct of others. During the child's early social intercourse with other children a not inconsiderable amount of time is devoted to interchanges of these attitudes and opinions. They enter into the child's social behavior and they influence its direction. In this way self-criticism develops hand-in-hand with mutual criticism, as the child acquires attitudes toward his own behavior that he is deriving continuously from other children and from adults.

From the standpoint of paranoic developments, the most important single feature of such social growth is the adequacy and the readiness with which a person becomes able to change his attitudes toward the conduct of others and toward his own conduct. In any given situation, differing attitudes give differing perspectives. Skill in shifting from one attitude to another makes it more likely that one will get a good all around picture of what is going on than is the case when one merely holds rigidly to a single attitude. Action can then be in terms of more than one single perspective. By being able to take temporarily the role of another person while ruminating over a given situation one may arrive at quite different conclusions regarding it. Where several roles in succession can be adequately taken, it is possible for a genuine



choice to occur. Compromise in action is more likely when it is the outcome of either discussion among different persons or of role-taking in a single person's talking and thinking. The paranoic action is typically uncompromising.

It is very evident in discussions among half-grown children that skill in role-taking, *i.e.*, in taking the attitudes of others and thus having their perspectives in one's own behavior, varies as much as do other social skills. As a person goes from childhood to adulthood there seems to be a general increase in the adequacy of such behavior, the adult usually being able to see matters when necessary from a child's standpoint more successfully than most children can take an adult's point of view. Of course the adult has already been a child and that helps to account for his greater social agility, but it is not the whole story. Most of the average adult's attitudes toward persons of the opposite sex, toward strangers and foreigners, or toward different customs and ways of doing things, show greater breadth and versatility than do the average child's attitudes. This is largely a matter of the development of social maturity; and social maturity progresses in different individuals at different rates and to quite different degrees.

Normal adults show wide variations in social interchange, in the ease and the completeness with which they can take the attitudes of others and see things from more than one perspective. They show great differences in their readiness to change a fundamental attitude once taken, or to give up a conclusion of personal importance once they have reached it. These characteristics of adults represent the resultants of a multiplicity of as yet unanalyzed factors, operating in countless situations involving other persons and their actions and reactions. So in any community group we differentiate persons who are habitually frank and confiding from others habitually secretive and reserved; we recognize those who can be flexible and relaxed in discussion and others who are more rigid, tense and insistent. We can always find persons who get on one track and stay there, either harping on one theme in public or brooding on it in private. We find uneasy sus-

picious men and women who lack skill both in grasping the attitudes of others toward them and in shifting their points of view to gain perspectives they need. These latter traits may appear to give such persons unusual singleness of purpose, but in a pinch they can become the gravest of liabilities.

Of course, there are some areas of action and attitude which almost all persons of a given culture tend to exclude from discussion. Such socially taboo subjects, sex behavior for instance, have much less chance of getting well worked out for the individual because of this stricture. In spite of the important part they may play in individual thought and action, the organization of taboo attitudes must remain relatively poor and asocial. This sharply restricted interchange allows only a simple fixed perspective which leaves individuals in these areas of action and thinking socially unresourceful in times of personal stress. During childhood the effect of adult displeasure, rebuff and ridicule greatly reduces social interchange on forbidden topics, and so drives them from the public social field of talk largely into the restricted personal field of thinking, where they can have only a very limited social validity. But there are many other areas also, beside the socially taboo, that get driven out of the social field before they have had a chance to be well worked out—for example, personal defects and peculiarities, or half-suspected trends and weaknesses, whether these are obvious to others or only believed to be so by the individual himself.

In both the socially taboo and the individually taboo, the circumstances that excluded these matters from social interchange continue to prevent their return to the public domain, that is to say, they keep them from being shared with others. Many of these difficulties do not reappear as factors in behavior; but others persist as areas of special sensitiveness in the individual, and these can become starting points for trouble later on. We recognize in persons whom we work with and know well that there are some subjects we simply cannot touch upon without unusual difficulties arising. In every little community there are certain individuals of whom we speak as being 'very sensitive,' because they have so many

areas that must be skirted gingerly if troubles and misunderstandings are to be avoided. This 'sensitiveness' does not necessarily imply that they have even average regard for the personal weaknesses or the welfare of others; they may be unusually inaccessible to others' needs and sensitivities. When we speak of a person's being 'sensitive' in this way we mean only that he reacts selectively in certain areas of social response very readily or excessively.

The effective environment of any individual gets organized through his own *responses*. While at the biological level and among structurally unlike species this organization will depend chiefly upon differences in receptor sensitivities, in similar species it rests more heavily upon what the effector organization makes possible. The deer and the mountain lion, for instance, have different environments largely because of the way each acts upon his surroundings. In any individual animal, as he develops more and more differentiated response organizations, he will react more and more *selectively* in his surroundings; to certain aspects of them he will show a ready ease of response, while for other aspects he will develop little or no specific response. It is in such differential response organization that any animal develops his special sensitivities.

The same holds true for the human species. The 'sensitiveness' of the artist to color, of the physician to physical signs, and of the reformer to signs of evil all rest upon special organizations of response which have in turn organized certain happenings of the surroundings into the artist's environment, the physician's or the reformer's environment. Things pass unnoticed for one that arouse the other at once to interested activity. A group of responses is ready to go off in one and not in the other; and this response sensitivity, which is not primarily a receptor affair, can create different worlds for different persons. When we say the world has 'special meaning' for the lover, the scientist, the soldier or the mystic, we are referring to the special ways of reacting each has developed in relation to surroundings that are common to them all.

A woman stepping out of her home in a new-fangled hat illustrates a special sensitiveness, based on her attitude toward her own appearance, and preceded by some ruminating about it. She steps out all ready to respond to any hints that her misgivings were justified. Anything that looks to her like ridicule or surprise in a passer-by will touch these response patterns off. A man smiling to himself about something clever he has just done will seem to this woman to be passing judgment on her hat, although actually he has not even noticed it. The more preoccupied with her appearance and the more insecure she is about it, the more likely she is to think signs and gestures are directed toward her and the more likely to find the laughter of others apparently referring to her. Her sensitiveness and her preoccupation serve the function of selecting fragments of the behavior of others and organizing them into a fictitious social pattern of which she seems to be the focus. The social pattern is fictitious because the attitudes she imagines she sees in others are her own; they are not shared mutually and are therefore private patterns and not social ones.

A woman can go home, take her hat off and get back her confidence. Other inadequacies and insecurities are not so easily dealt with. Real or fancied deficiencies and defects, persistent failures and rebuffs, behavior peculiarities, or even a bad conscience can build up extensive organizations of sensitiveness. This sensitiveness operates in the manner of an anticipatory 'set,' which selects things out of the common surroundings simply because the organism has become more ready to react to some things than to others. In common speech we say things have 'special meaning' for such a person; but this special meaning goes back to his organized readiness to *respond* in some special way. If the woman in the new-fangled hat can discuss her experiences, she can change her attitudes and make another sortie which may be successful and so end the incident. Peculiarities or defects of personality are less detachable than a funny hat and harder to surmount. If in addition there is a lifelong difficulty in sharing one's personal attitudes with others, this will block



the important highway of social communication, along which help must come if it is to come at all. The lone person broods instead of communicating; the sensitive person finds corroboration for his fears in the direction of his sensitiveness; the person with defective role-taking habits can see things from only a very limited perspective and his conclusions are therefore irresistible, final and without genuine choice of alternatives.

At this point I shall introduce a rather typical case history, not indeed to prove anything, but to illustrate certain important aspects of paranoic developments.

The patient is a middle-aged unmarried male. According to him, his difficulties came on suddenly following a quarrel over a racing bet placed with 'bookies.' After the race he claimed to have put his money on the winning horse while the bookies insisted he had not. After a few drinks elsewhere, he returned to the scene and got into a noisy altercation with the 'bookies' in which he insulted them, threatened them and invited them out into the street to fight. After he had returned to the hotel where he lived, he began thinking it over. He remembered stories of nation-wide gangster protection given to 'bookies,' and the more he thought about it the more dangerous his attack upon them seemed to him. He looked about the hotel lobby next day and noticed some strangers watching him closely. They seemed to be making signs to each other. During the morning an automobile full of men stopped in front of the hotel door and he became convinced he was about to be kidnapped, tortured and killed. Now he began to notice strangers and loiterers everywhere and they all seemed to be keeping track of him. He was a 'marked man.' He barricaded himself in his room and told a relative over the telephone about it. That night he fled across country in his car; but incidents *en route* made him realize he was being followed, and spurred him on. In one city, for instance, he saw a policeman examining his auto license. Under the circumstances that could only mean the police were in league with the gangsters. In a shoeshining parlor the attendant eyed him narrowly; "the grape-vine system was catching up."

Finally he equipped himself for suicide; but by this time he had joined relatives who suspected as much and guarded him. They persuaded him to come to a psychiatric clinic.

In the clinic he felt for some time secure. He was well-oriented, circumstantial but clear in his talk, his conduct was formally correct, and while he preferred his own company, he was pleasant and courteous to others. He made frequent allusions to things in his past that he would like to get cleared up; but he could bring himself only to go over some unethical business procedures. He persuaded a new physician to let him ask a local pastor by telephone to visit him at the clinic; and to the pastor he told the story of his recent difficulties, and made an appointment to talk about other things without consulting his physician. The more the patient thought about it, after the pastor had gone, the more he suspected that he had been unwise to confide so much. He reflected that the pastor was somewhat dark-skinned and foreign-looking; and then he realized that his telephone call had probably been intercepted by the gang who had sent a confederate around pretending to be a minister of the gospel. There was a violent resurgence of fear, which he kept to himself, and another suicidal plan that was nearly successful. He was unable to adopt any other attitude toward the pastor's visit, nor was he willing to attempt in any way to unify or contradict his conclusions. They were true for him. He was convinced that transfer to a government hospital was the only thing that could save him now. Transfer was finally arranged. His prognosis is poor.

My first point of attack in such a situation is the same as it would be in a medical history: Has anything like this happened before? Is it an isolated incident? Does it have any background that makes such an extraordinary result from such relatively trivial circumstances intelligible? In the case of a cerebral accident or a parenchymatous infection the answer is simpler and more direct; but the history revealed nothing in these directions, and the neurological and laboratory examinations were negative.

The personal history, however, was by no means negative.

His childhood had been motherless and he was shifted about from relative to relative, from farm to farm and from state to state, with never a settled place to live in, now with his siblings and again separated from them. He grew up a lonely, brooding, insecure child with an ability to get along well in casual contacts but lacking real friends with whom he could share confidences and exchange perspectives. In early adult life he drifted into vocations that permitted him to work alone, and in which he was uniformly successful. He has repeatedly felt himself discriminated against, watched and trailed by his employers' or his competitors' operatives. His reaction to this supposed treatment was to make drastic moves to some other field, without consulting others and on the basis of his own ruminations plus some circumstantial evidence that he picked up by watching and checking on others. At one time he was employed for awhile in detective work. This he disliked, but he felt that while it heightened his suspiciousness of others, it also made him better able to recognize what was going on. He had always felt the need to confess and confide in someone but had never been able to accomplish it.

With this case before us, other questions remain to be considered. Why in some persons do misinterpretations arise so easily and seem to stick? How do they start up? Why do they become extended, systematized and dominant? What are the circumstances of their breaking out into overt action? Misinterpretations are constantly appearing in everyone, but most of them get corrected. In the social field, where the attitudes others have toward us is the important thing, such correction comes about by free interchange and discussion, *i.e.*, by putting oneself as far as possible in the place of the other person and so having his point of view, by shifting from one perspective to another, and often by modifying one's own conclusions or even by adopting those of one's opponent.

Such maneuvers involve social skills, and individuals vary widely in their mastery of such skills. As also in the acquisition of language habits, one needs to begin these processes in early childhood and to continue them actively throughout life

if ease and adequacy in social interrelationships are to be achieved. In the background of paranoic persons there are always circumstances unfavorable to the development of such skills. By the time adolescence and adulthood have been reached, inadequate role-taking and limited perspectives have built up a socially inflexible, one-track person in whom opinions are apt to crystallize easily into unshakable convictions. Habits of reticence and concealment imposed earlier in life mean a real inability in adulthood to share experiences and attitudes. Lone thinking, preoccupation and puzzling are apt under these circumstances to take their place. Things seen and heard are thought about, interpreted and brooded over, but not shared with other persons. Conclusions need then only be plausible to the person arriving at them to be valid for his way of thinking. Under the directive influence of some personal stress, this watching and listening and brooding can develop cumulative misunderstandings. Hypotheses and deductions are then inevitable; but the lone individual lacks the usual checking over with other persons which might modify and correct them (4). He goes on from one deduction to another until he has built up a systematized explanation of things.

There are always incidents in the immediate surroundings or in what one remembers that will corroborate a growing delusion or a pet theory. Under such conditions the evidence is always selected, the dice are loaded. For, what an individual under some personal stress will see and hear, or what he will recall, will depend mainly upon what he is sensitive to. Personal deficiencies and defects, whether publicly known or privately believed, provide attitudes that are especially receptive to signs of slight and injury. Where the character of the personal problem, or of the plans growing out of it, make exposure or reprisal seem likely and imminent to the individual, he begins a furtive process of searching the surroundings for signs, warnings and threats. This is apt to bring the whole drama out into the objective social field.

We now have the paranoic paradox. Operations that have validity within the limits of private or organismic thinking are



carried out by him into a social field where they have no such validity. In the older mentalistic psychologies this was described as merely 'projecting' one's ideas into another person, much as one might throw a dead cat into a neighbor's yard to rid oneself of a nuisance and a responsibility. But when a paranoic person falsely ascribes functions, attitudes and intentions to other persons, he does far more than merely to put his thoughts into them. He sets up hypothetical interrelationships between other persons and himself, and he organizes them functionally into a *pseudo-community*, made up of objective persons with imaginary functions. These imaginary functions are built up out of fragments of the social behavior of social persons. The fragments of behavior are misunderstood by the paranoic in the direction of his expanding system. The actual movements, remarks and other actions of people around him become cues, signals, threats and warnings within a pseudo-community of plotters. Out of these raw materials in his surroundings the paranoic organizes a functionally interrelated environment, of which he is the focal point. Its pattern develops from his sensitivities and preoccupations, as well as from the more accidental character of the corroborative detail he finds about him. Eventually he reacts overtly to this whole structure. In most instances he begins to take protective or aggressive measures and counter-measures, until the whole thing finally erupts into the social field.

So long as a person's reactions are confined to fantasy, wondering and observing, he does not commit himself; he can always retrace his steps without suffering retaliation from others. As soon as his reactions implicate others, however, the consequences of what he does can no longer be controlled by him as they could be in fantasy. It is this change in the development that gives the false impression of a sudden onset to the disorder. Actually, it is not the onset but the recognition by others of the disorder that is sudden; it has been brewing a long time.

Usually the paranoic is first recognized as abnormal when he begins operations in the socially shared field that corre-

spond to developments, often of long standing, in his personal or organismic field. He has put together a *dramatis personae* made up of individuals in his surroundings who function for him as a community whose common aim seems to involve his vital interests. His own reactions to this pseudo-community carry him actively into the social field where these persons and their behavior fragments are operating. Since they are no genuine community and do not share with him or with each other these relationships he has postulated, his behavior seems unintelligible to them. The final outcome then is either that he acts detrimentally upon his surroundings and faces retaliation, or else that his community tolerates, ignores or isolates him because of his conduct which cannot be included in its own activities. Very occasionally, by good fortune, his behavior may fill some need in an ongoing cultural development and make of him for the time being at least a distinguished person and, though rarely, a leader of men.

#### SUMMARY

1. Current theories of paranoic developments attempt to localize the disorder either in the nervous system or in a 'psyche,' and assume the opposition of a real and an unreal world. An approach is outlined here that differentiates only between social and individual behavior, and ascribes delusion to defective socialization.

2. One's attitudes towards one's own conduct get organized through the reactions of others; and the ability to take and shift perspectives depends upon how well habits of role-taking are developed in the individual.

3. Ability to shift and to share perspectives is a form of social skill in which individuals vary greatly; under conditions of personal stress, perspectives that are relatively fixed and one-tracked constitute a grave handicap to adjustment. This is especially true among socially taboo matters or those of personal sensitiveness.

4. Surroundings and happenings are organized into an effective environment by one's own responses; and personal sensitiveness here means readiness or ease of response in some

direction that operates selectively, in the manner of an anticipatory set.

5. Hypotheses and deductions in the asocial individual lead only to further hypotheses, without being exposed to the modifications which the opinions and perspectives of other persons might otherwise have brought about.

6. Eventually a *pseudo-community* is built up by the paranoic person's reactions to his own preoccupations; individuals around him and fragments of their behavior get organized into an expanding system of delusion. The person begins operations in the social field that have validity only within his own individualistic thinking. Then his measures and counter-measures, in relation to the supposed action of others, bring him into conflict with his environment. The final outcome of such a development is usually that the community isolates him in some way from its active life.

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## EMOTIONS AND MEMORY<sup>1</sup>

BY DAVID RAPAPORT

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Psychological theory has often disregarded in the past and still often disregards certain fields of psychological phenomena. We can hardly enumerate here how the investigations of personality were long left to the writer of belles-lettres or to the historian, how the investigation of many psychological phenomena is still left to the linguist, or how the investigation of many psychological problems is left to the psychiatrist and anthropologist. In the following we shall see that a similar compartmentalization exists in the field of memory phenomena. Theories of the Ebbinghaus type view memory as a specific function which can be investigated by experiments in learning and by testing the retention of learned material. This view implies the assumption that the simpler and more nonsensical the material, the more accurately and truthfully the laws of memory functioning will show themselves in the experiment. These theories disregard the fact that memory phenomena (imprinting, retention, recall, recognition) occur not only in the setting of purposeful learning, but also *continuously* in everyday life, and that in fact all psychic processes imply one phase or another of memory functioning. They therefore neglect the investigation of memory phenomena in their natural setting as well as of pathological memory phenomena. Janet aptly said:

The psychologists in their descriptions admit of no other elementary phenomena of memory than conservation and reproduction. We think that they are wrong, and that disease decomposes and analyses memory better than psychology (9, p. 102).

The problems of amnesias and paramnesias, the Korsakoff syndrome etc. have remained in the psychiatrist's domain.

<sup>1</sup> Paper read in the Psychology Section, 1941 Dallas Meetings of the American Association for the Advancement of Science.



Hypnotic memory phenomena have been left to the hypnotists' care. Everyday forgettings, slips of tongue, and the universal amnesia for the greater part of childhood experiences were a no man's land until Freud and other psychoanalysts became interested in these phenomena. There have been significant discoveries made in these fields, but these discoveries have never been integrated or consolidated along the lines of general psychological findings or of theories of memory. Of the few attempts at integration, those of the psychologists Ray (17) and Sears (18), in the 1930's, remained a mere beginning, while those of the psychiatrists Gillespie (8) and Schilder (1) cannot boast of much better results. The fact that memory is but one of many aspects of our thought processes and not an independent function was not made sufficiently clear in these attempts, nor was experimentation sufficient to clarify the laws governing either the normal functioning of that aspect of our thought processes which we are accustomed to call memory or of its disturbances.

In the following we shall attempt to describe the progress made by experimentation and clinical observation towards a new understanding of the memory processes.

In the experimental field the main headway towards an understanding of the functioning of memory in everyday life was made through investigating *the influence of context and of attitude on learning and memory*. Usually the contexts used were well delimited, in contrast to everyday life where usually no delimitation exists. Also the attitudes were conscious and formulated in verbal-logical terms, while in everyday life no such formulation can be found and attitudes are usually affect-bound. The role of the context in these experiments was to constitute a similar or a heterogeneous background either of which could be *meaningful* or *meaningless*. The contexts of everyday life, however, play the pianoforte of attitudes which they elicit or rather interact with, thus *determining* and *organizing*—not merely *facilitating* or *inhibiting* imprinting and recall. The attitudes usually investigated were 'like-dislike,' and 'pleasantness-unpleasantness' of the learned material and of its context. Other attitudes were elicited by

reward and punishment, by obscene, religious, etc. connotations of the learned material. It was assumed that in the experimental situation these attitudes were subject to inter-individual agreement. In everyday life, however, attitudes are subjective matters varying with individuals and depending upon a wide range of personal constellations running the gamut from intellectual judgment-like attitudes to affective, instinctive strivings deeply rooted in the personality of the subject. These experiments frequently claimed to have demonstrated the influence of emotions on memory or to have proved or disproved the Freudian theory of repression. Though these claims were unwarranted, the experiments at least kept alive the problem of a more general theory of memory.

As long as memory experimentation was essentially based on the Wundt-Ebbinghaus presuppositions or on the frame of reference of conditioned response, the attempts to investigate the functioning of memory as one aspect of the thought processes of everyday life were, as we have just described, generally limited to experimentation on contexts and attitudes.

A new attack on the field of memory, designed to approach memory function as it exists in everyday life, was started by Ach, according to whom associations and memory revival obey not only the law of strength of associative bonds but also that of a 'determining tendency.' Lewin (12), disagreeing as to the nature of this tendency and its relation to the associative bonds, showed the central significance for learning and memory of a specific type of attitude he called 'readiness to reproduce,' or 'readiness to rhyme.' On the basis of this finding he later developed the theory of tension systems, according to which a decision, an intention, etc. corresponds to a psychic tension system which becomes discharged only when the decision or intention is carried out. If the memory of the objectives of these decisions or intentions is also dependent on these tension systems, they will be better when the intention has not yet been carried out than after it has been. Zeigarnik's (20) often verified experiments proved that this deduction was correct.

Thus, experimental proof was offered concerning a phenomenon frequently observed in everyday life. The main deficiency of the Lewinian experiments was that they did not make quite clear just which of the many memories pertaining to an interrupted task, that is to say, to a tense psychic system, have an advantage in remembering. The experiment dealt merely with the remembering of the names of the interrupted tasks—while in everyday life the tension systems exert an influence on many related psychic contents. Thus, their relation to the functions of everyday life still remains questionable, though some experiments on substitute activities appear to attack the problem mentioned.

Whereas in order to study 'normal' memory functioning, general experimental psychology investigated context and attitude, and Lewinian psychology investigated the link between memory and tense psychic systems, Gestalt psychology investigated the issue of 'meaning.' The classical and the conditioned response type of memory experiments used nonsense material for the most part. Gestalt psychology attempted to show that the memorizing of any material, even that of nonsense syllables, utilizes an organizing process by which some kind of structure, or in other words, a 'meaning,' is given to the material. But the Gestalt psychologists unfortunately restricted their investigations to *logical* meaningfulness. They showed that reproductive memory function is most similar to what happens in everyday life when it deals with meaningful material, in the reproduction of which it essentially acts as problem solving, or productive thinking does. Certainly, our everyday thinking and that aspect of it which we are accustomed to call memory are penetrated by logical meaningfulness; but if one were to try to represent our thinking and memory functions as satisfying merely *logical* meaningfulness, he would soon have to call the logically flawless reasoning of the paranoiac 'sane,' because he would neglect the *affectively* meaningful factors which lead to the paranoiac's deluded presuppositions on which his logic is built. The logical meaningfulness that Gestalt psychology came to investigate is one of the most important aspects of memory

functioning, but it must not be forgotten that it is only one aspect among many.

Bartlett's (2) experimental attack on the everyday function of memory may be considered a bridge to clinical observations on memory. He experimented mainly with the reproduction of stories and interpreted his results as showing that the core of reproduction is always an attitude, an emotional-affective tendency, and that reproduction is essentially *production* around this core: an attempt to justify and to convey this attitude. His experimental results corroborated views expressed previously by theoretical psychologists such as the personalist W. Stern, or the kinæsthetist R. Mueller-Freienfels. Contrary to such conceptions of memory functioning as those of Ebbinghaus, Wundt, and G. E. Mueller according to which an idea in consciousness is followed by that idea having the strongest associative bond to it, what actually happens in the memory function of everyday life corresponds to the finding of Bartlett: an attitude, an affective or emotional striving expresses itself by reviving and organizing memory traces.

The available clinical observations on memory functioning can be grouped from either the point of view of subject matter or that of method. As regards subject matter, we may distinguish between normal and abnormal memory phenomena. Clearly, the study of memory functioning pertains to the organization of memories, of which organization memory distortion and forgetting are examples. As regards method, there were the mere clinical observations as distinguished from clinically oriented experiments, only the latter of which can justifiably be called controlled observations of the memory function in general and of the gross pathology of memory.

The greatest mass of observations concerning memory function was accumulated by the psychoanalytic school. These observations were crystallized into several concepts, such as parapraxis (6), repression (10), and primary mechanisms (7; 13, pp. 243-247). The theory of parapraxes, of which everyday forgetting is only an example, maintains that slips of the tongue and slips of memory as well as forgetting are due to an unconscious striving interfering with the tend-



ency to revival of the memory in question. The theory of repression maintains that a striving which would be painful to consciousness is therefore kept out of consciousness and that memories connected with the striving become themselves unconscious, that is to say, are 'forgotten,' but can be recovered under proper conditions; in other words, there is a tendency to avoid the awakening of pain through memory. The theory of primary mechanisms maintains that ideas can replace each other, can merge with each other, and can be displaced to express and yet through distortion to conceal basic strivings of the person. These theories have found some experimental substantiation in the experiments on substitution conducted by the pupils of Lewin (13), in the experiments of Bartlett (2), and in experiments to be referred to later on.

The observations concerning the pathology of memory are so manifold that they can hardly be summarized here. The main bearing on memory theory of the investigations on retro- and anterograde amnesias, multiple personalities with alternating systems of memory, loss of personal identity or fugue states, and the Korsakoff syndrome may be summed up as follows (16):

First, the investigations on loss of personal identity and fugue states show that attached to memories there is a personal sign, an attitude of personal identification, which if lost or withdrawn from a whole period of experiences results in an amnesia of a particular type (loss of personal identity or fugue state). It probably plays a role also in the difference between accumulated knowledge of impersonal character and the memory of personal experiences and is an important factor in recognition. In multiple personalities, two or more systems of such attitudes of personal identity are probably responsible for the reciprocal systematic amnesias. This personal sign, or attitude of personal identity, is a link to Bartlett's experimental results as well as to those results of Lewin which maintain that attitudes or affective tendencies are the essential factors in recall.

Secondly, the investigations on the Korsakoff syndrome show that memories have a 'temporal sign' the loss of which

causes impairment of remembering in everyday life and peculiar reduplicating paramnesias and amnesias, although at the same time, learning and immediate recall may be relatively intact. The memories emerge inappropriately and may emerge incidentally, but are unavailable at the proper moment. Such a factor of temporalization probably plays an important role also in the difference between knowing, remembering, and recognizing, and may probably underly the phenomenon of '*déjà vu*'. Although the relation of the loss of temporal sign to the affective disturbance present in the Korsakoff syndrome has not yet finally been clarified, it appears probable that the disturbance of temporalization is related to a disturbance of the dynamics of the strivings of the individual.

Thirdly, the investigations on amnesias by means of re-learning experiments show saving, while common hypnotic as well as drug-hypnotic experiments show recall. These facts demonstrate that in functional and even in organic amnesias traces of earlier experiences are not simply lost; rather they are unavailable for recall. Thus the Ribot-law—so widely accepted as explaining memory loss—which maintains that the more labile recent memories and not the old and well-entrenched ones are lost, becomes invalidated. It probably will have to be replaced by a law to the effect that memory traces have an architectonic in which the attitudes link earlier traces to later ones and that when these attitudes are withdrawn they necessarily follow this architectonic rather than a temporal sequence.

The clinically oriented experiments have attempted to show how individuality, personal strivings, unconscious motives, and typical psychiatric conditions influence memory material. We refer here mainly to three types of experiments, (1) story reproductions, (2) reproductions of drawings, and (3) hypnotic experiments.

Of the first group, the pioneer experiments of Koeppen and Kutzinsky (11) showed the memory distortions typical for different psychiatric categories. These were followed up by many investigators inquiring into more individual character-

istics of memory changes, and showing how symbolic, condensing, and concealing distortions of memories come about. More recent experiments, like those of Despert (4), show how fairy tales heard in childhood are changed by organizing memory to fit the needs of the individual recalling them.

The experiments of the second group leaned partly on the Gestalt-psychological experiments of Wulf (19) on changes of drawings in reproduction. It was shown that in cases of brain disease and of functional psychoses the reproduced figures degenerated to more primitive forms (3). Other experiments of this group, like those of Poetzl (15), Malamud (14) etc., showed that in tachystoscopically presented pictures, parts painful to the subjects are omitted in the immediate recall.

In experiments of the third group, of which Erickson's (5) experiment is an example, strivings and tendencies were implanted into the subject by posthypnotic suggestion. By demonstrating that attitudes and strivings assert themselves by organizing memories, these experiments verified the basic role of attitudes and strivings in memory functioning. The memory organization they bring about to express themselves is one in which the Freudian mechanisms of symbolism, condensation, and displacement, the dynamics of trace-systems of Gestalt psychology, as well as the influence of contexts and attitudes are all interwoven.

What then is the field a new theory of memory would have to embrace and what are the main lines along which such a new theory can be crystallized?

A new theory of memory will have to embrace not only the problems of rote learning and those of meaningful learning, but also the problems of memory functioning in everyday thought processes (of which logical thinking in problem solving is only a special case), and the phenomena of memory pathology.

The main lines along which such a theory may crystallize are the following:

(a) remembering and forgetting are essentially emergence and nonemergence in consciousness, that is to say, expressions

of whether a content is available or unavailable for emergence in consciousness;

(b) forgetting as a result of decay may or may not exist—in either case it is of negligible importance. Rather, the essential fact is that dynamic factors cause memories either to be delivered into consciousness or to be prevented from emerging there;

(c) drives, strivings, motives, needs, affects, emotions, tension systems, determining tendencies, attitudes, mental sets, etc., are names for the different dynamic factors responsible for the organization of memories. This organization may result in the facilitation or inhibition of reproduction or may result in transformation, distortion, symbolical substitution, condensation, or displacement of memories. A theory that purports to explain the memory phenomena of everyday life as an aspect of thought processes can only be built on the basis of investigations that reveal the relation of these dynamic factors to the phenomena of memory organization.

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## THE BOTTLENECK IN PSYCHOLOGY AS ILLUSTRATED BY THE TERMAN VOCABULARY TEST<sup>1</sup>

BY HARRIET BABCOCK

Two groups of psychologists have been culpable in the general failure to appreciate the importance of the Terman vocabulary test in studies of mental organization and to give it its due place in clinical and research work. One is represented by the clinical psychologist whose early training has not given him an opportunity to study normally functioning persons by the same methods as those he uses with the abnormal, and who, because of this unfamiliarity with a sufficient number of normal minds, tends to attribute too many failures to lack of opportunity rather than to personal incapacity. The other offender is the academic psychologist who has often had no experimental contact with either normal or abnormal *individual* minds of different ages and different degrees and types of mental functioning, and as a consequence lacks a sound basis for judgment in such matters.

Because of this lack of background which is necessary for the evaluation of psychological work, significant findings often have either been completely ignored or else have been misinterpreted. For this reason, among others, the significance of great discrepancies between vocabulary scores and scores on other kinds of tests has not been generally grasped. Reported vocabulary successes made by border-functioning subjects, especially those of the young *præcox* type, have usually been interpreted as due to special 'verbal intelligence'—as if relatively higher verbal scores were a distinguishing symptom rather than a superficial manifestation for which some more basic cause must be discovered before there can be any understanding of the condition. 'Verbal intelligence' not only fails

<sup>1</sup> Other vocabulary tests would serve the same purpose if they were standardized in the same way on developing intelligence.

to explain, but it also ignores the fact that vocabulary scores range from low to very superior among normal and border-functioning persons alike.

Intermediate states of mental malfunctioning are not so much characterized by 'verbal ability' as by *weakness in the apprehension of new data* with attendant weakness in both perception and learning. As a result most verbal scores tend to be higher than the individual's other scores. In fact, work with normal persons on whom the same proceedings are used as on the abnormal, contradict so many preconceived ideas that in some clinics a vocabulary test is always used even on subjects who are most handicapped as to educational privileges. The results are not necessarily taken at their face value but are welcomed for the unexpected light they often throw on a problem.

Terman's discovery of the relation of the vocabulary test to developing intelligence might not have changed the general idea that it was dependent upon age and experience, since opportunity to know more words is of course greater at each life age, if he had not also found out that children who did not succeed in school knew a smaller number of critical words than children of *the same age* who did well in school; and that the ones who did best had the largest vocabularies, often even when they were younger and when a foreign language was spoken at home.

In a study of problem children in an institution for the insane—of children deprived of the advantage of formal education—Lydia Levy (8) found that in most cases the vocabulary scored higher than the other tests and was the best indication of potential mental level.

Recent criticism has been based on the idea that the Terman vocabulary test is not appropriate to adults. This conclusion ignores the facts that the content of almost any test can be made acceptable to adults if given in a suitable manner; and, furthermore, that if the units used are to have real significance as to the essential factor in intelligence, they must be based on developing abstract-verbal ability.

In a study of the Pintner-Patterson Performance test (4),

it was shown that many subjects who were handicapped by unfamiliarity with the language often did better on the untimed Terman vocabulary than on simple concrete tests which were timed, especially if they retained some of the effects of previous psychotic conditions. That it measures some central core of mental ability, which is unaffected by the time factor in a normal population, was also shown in a study of the Army Performance Scale (5).

Unexpected success with the vocabulary test is common. For example, persons reported as mentally deficient because of their poor educational history, in places where education is not compulsory, often give average or higher vocabularies even when other apparently easier tests score at very low levels.

Evidence of the test's close relation to level of intelligence in adults as well as in growing children and its lack of relation to the pure fixation phase of memory was furnished when normal adults were used in a study of paretics (2). In the course of this investigation the vocabulary scores were found to be normally distributed, and scores on the examination correlated highly with the vocabulary in both the parietic and the normal groups. One result of this study was to show that discrepancies between vocabulary scores and scores on the mental efficiency examination were directly related to ability to make adjustment outside of institutions for the insane.

The next evidence of its validity and its clinical value was furnished by the results of a study of dementia præcox (1) in which there was a similar distribution of verbal intelligence and relation to paroleability. The measured impairment of the non-paroleable group was less than that of the paretics, however—a fact partly to be explained by the difference in age and in vocational and professional success before commitment. In this study clear recognition was given to the impossibility of getting reliable vocabularies in cases so extremely deteriorated that verbal level could not be estimated and discrepancies could not be measured. The argument often advanced that it is not a valid indication of potential mental level in cases of advanced deterioration, because ability



to give definitions also deteriorates with the other capacities (7), is true. This fact however in no way impairs the value of the vocabulary test for the large number of cases who are not markedly deteriorated, nor does it argue against its use in research with normal persons, nor with those in the intermediate states of malfunctioning—a group which is particularly illuminating in its disclosures as to the nature of mental organization.

The value of the vocabulary has also been minimized and distorted by the idea that it is merely a 'learning' test. This is still a generally accepted explanation of its function and one which has been frequently repeated since the time that mental level was first controlled by the use of the vocabulary in studies of mental deterioration. There is no reason for such an interpretation. Superior vocabularies are often given by persons who have about the poorest learning ability of any of the border-functioning subjects. Also it is an interpretation which does not consider the level of ability necessary for the understanding of words. In spite of the incontrovertible fact that the words were once learned—we can point to little that was *not* once learned—the Terman vocabulary test is not essentially a test of learning ability for the one essential of a learning test is that there be some way of knowing how the ability of a person to learn compares with that of other persons of the same level of intelligence, either by the control of time or of the number of repetitions required. Since the vocabulary is acquired without conscious effort, we have no criterion for deciding whether the learning ability was good or poor, though we can tell whether the vocabulary learned be of an inferior or superior quality.

The learning of a vocabulary without conscious effort depends mostly upon potential level of intelligence and upon emotional congruity. It requires ability to understand the particular distinction or principle represented by the word in question. The vocabulary is first of all a test of understanding and discrimination. A poor learner often remembers a new word after once hearing it, more because of his interest

and the number of associations it immediately stimulates than through conscious repetition and effort.

The satisfactions of verbal discovery are as real as other discoveries and other satisfactions of later life. It is a great experience for a child of superior mental potentialities to hear a word for something of which he has vaguely been aware for a long time and for which he has had no exact tool of expression. Most of us have known intelligent children who, on first discovering that every object has a name, take great delight in finding out the name of everything they see, and later in trying out all the names they know for one person or thing.

It is evident that when a vocabulary test is used with tests of learning, two learning tests are not being compared, but two aspects of general intelligence which score closely as to levels of ability among normal persons, but which show various degrees of deviation in border-functioning conditions.

A later argument against its validity as a measure of mental level is that there is too wide a range in the quality of the definitions which are accepted as correct. This criticism ignores the fact that both the choice of words and the criteria for scoring were empirically derived, and that mere argument cannot change the proved relationship.

The years have only served to strengthen Terman's original evaluation of the vocabulary test. In spite of opinions expressed to the contrary, it is a test that seldom fails to throw light on a person's mental capacity *when used by experienced psychologists*. Varied experience under controlled conditions is an essential pre-requisite for judgment as to the reliability of a vocabulary score, and also for ability to use it in research. *Persons who do not have sufficient education and experience in psychological surgery should not be allowed to fool with its tools.*

Throughout the test's history from the first revelations when used with children, through all the research with border-functioning persons, results obtained from it have come each time as a surprise. Their value has been repeatedly denied by the inexperienced, and has been repeatedly rediscovered as the test was more often used in a normal adult population by

experienced examiners. Other vocabularies have been devised and other kinds of verbal tests with the idea of appropriateness to the adult mind. Most of the other kind of verbal tests however require more concentration and mental control and are too apt to be failed in states of mental malfunctioning. Also, most other vocabulary tests have not been standardized on developing intelligence and, because of the way in which the words were selected, the *number* of words more than the *kind* has often had too great an effect on the scores. Consequently when put to the test they do not show a sufficiently close relation to mental level.

The fact is that the two phases—facility of expression, with prompt use of the words a person understands, and level of verbal understanding—while highly correlated, are two different aspects of verbal ability. The one is more closely related to mental efficiency; the other to mental level.

In Wechsler's recent effort to standardize an examination suitable for adults, he clearly expresses his conversion to appreciation of the Terman vocabulary, which was the one he was using and which caused him to try to standardize another one. He writes:

. . . our experience has shown that factors of schooling, etc., influence the effective range of an individual's vocabulary much less than is commonly suspected. We have been much surprised to find that illiterates and even individuals of foreign birth who have acquired only a moderate amount of English are penalized far less by a vocabulary test than by many others seemingly less linguistic. Unfortunately, we did not come to a decisive realization of these facts until a considerable portion of our subjects had already been examined, . . . (9, p. 301).

It has long been recognized that physicians must be thoroughly familiar with normal anatomy and physiology before they can distinguish what is significant in sickness. It is now as apparent that psychologists should be equally familiar with normal minds of various types and ages before they can have a sound basis for noting and understanding mental weaknesses, or before they can appreciate the relative importance of tests and evaluate contributions to learning.

In considering the history of the vocabulary test since Terman's early work with it, one is struck by the fact that appreciation of its value comes, not so much from clinical workers who are apt either to accept or reject it as on *a priori* grounds, as from persons, who, after doing a great deal of clinical work, have for different reasons carried on controlled research which required the examination of large numbers of normally functioning individuals.

In each case earlier prejudgments as to its lack of validity for different special groups gives way to acceptance of it as a measure of potential mental level except in a very small per cent of cases. This was true in the study of paretics. In the study which first showed the value of the vocabulary test for the understanding of border-functioning mental conditions four years were spent in fruitless experimentation with different intelligence tests before it became evident that they all were invalidated as measures of pre-paretic mental level because scores were lowered by the mental inefficiency due to the patients' weakness in perception and in the apprehension of new data. That is to say, scores were adversely affected by the very factor which we were trying to isolate, or to control and measure. This was not true of the vocabulary test.

The mental symptoms of different clinical types as currently reported are unnecessarily confused because behavior which is a result of potential intelligence is not differentiated from behavior which is due to the psychopathic or unstable personality. Symptoms are still more confused because of the inclusion in clinical types of behavior symptoms seen in persons of very different degrees of mental impairment—as if behavior in mild cases could be like that of the greatly deteriorated, or as if the behavior of an intelligent person could be like that of a moron (except in the poor mental functioning) merely because the former could not make efficient use of his potential intelligence!

Obviously, the value of the Terman vocabulary test in mental analysis is not to be settled either by argument or by *a priori* reasoning, especially if the settling be done by persons unfamiliar with the problems of testing, or by experienced



testers who are insufficiently familiar with the mental functioning of a normal population.

*The correlations which have already been disclosed are facts, and as such need to be accepted and explained.* Acceptance of the facts already derived and careful consideration of the possible causes can throw needed light on the larger problem of mental organization.

We should consider why it is that in a normally functioning population scores of tests which have been timed, as in the mental efficiency examination, and many simple performance tests which can be understood at very low levels of intelligence, are higher at each higher vocabulary level. Also what the mental factor is that is essential to the easy tests and also correlates highly with the acquisition of an effective vocabulary in a normally functioning population.

We should try to understand why it is that when there are great deviations in these normal scores at each vocabulary level in an apparently normal population, these deviations are positively related to obvious personality types—the slow, with poor mental efficiency, to a generally introverted personality; the quick, with good mental efficiency, to extroversion and self assurance; while persons not definitely describable either way make efficiency scores close to normal medians—for persons of the same vocabulary levels.

We should also learn why it is that in the border-functioning group between normality and insanity there is not only a high correlation between vocabulary and timed easy tests, but also why, even though scores are higher at each vocabulary level, *the actual scores are lower than are those of the normally functioning group of the same vocabulary level.*

Also, if special adult tests are to be intelligently devised, we should first understand why persons of low vocabulary levels who can make satisfactory adjustment make relatively higher scores on easy tests than is to be expected for persons of their vocabulary levels, though the scores are actually lower than those of average or superior persons who are considered neurotic or emotionally unstable and cannot make satisfactory adjustment.

We should know why it is that, when *both* mental level and the degree of mental malfunctioning are controlled, characteristic mental defects have been brought out; as, for example the special motor inefficiency in mild catatonic states (6), which is not otherwise apparent.

Whatever the explanation may be, the facts point to the necessity for a revolutionary change in any psychological research which deals with human capacities. They show the necessity of weighting scores in units which correspond to developmental levels, the value of placing less reliance upon the statistical manipulation of group tests, and the importance of giving more time to the study of each individual who makes up the group.

Besides norms for the populations as a whole, there should be separate norms for the different abstract-verbal levels, for it is not only important to determine a person's place as to general mental ability in the population as a whole, but, in order to learn how efficient he can be in the use of his potential intelligence, it is also essential to know where he stands in the distribution of persons of his own mental level. This fact has been proved in the numerous studies in which efficiency scores had little or no relation to malfunctioning conditions, while discrepancies between efficiency scores and vocabulary scores were positively related to such conditions in border-functioning groups.

In fact if level of intelligence is not controlled as nearly as possible, there can be little certainty as to meaning of results because tests which are difficult for a person below average in intelligence may be almost automatically easy for a superior person; and tests which show differences in a superior group may show nothing as to quality of mental functioning in an inferior group who will scarcely understand them at all.

Failure to see the value of such results and to make use of them has not been so much due to the fact that leaders in psychology minimize the findings, as because most of them do not recognize problems which deal with human minds in their total functioning as appropriate subjects for serious research.

It is unfortunate that the lessons learned through the control of mental level which the vocabulary test has made possible should be ignored, and that the underlying significance of the findings to psychological theory has not been adequately appraised. This failure affects much more than psychology in the schools and in mental hospitals. Work based on its underlying principles could be used to eliminate the emotionally unstable from the armed forces. Even group tests could be so given and scored as to detect a large number of such cases, and to indicate many of the others who would need individual study.

Psychology, by making use of the level-efficiency concept in its relation to border mental functioning, now has a chance to make as great a success in the field of emotional instability as it made in the field of intelligence in the last World War.

The obvious lesson to be learned from the slowness of psychologists to see and make use of important principles which could have accelerated the practical value of psychological work, is that every psychologist should be required to have a minimum education in the study of both normal and abnormal minds—minds of *individuals*—so that the interrelations between the different more basic mental processes can be understood. A study of as few as one hundred subjects, provided that each subject were given three or four standard intelligence tests so that there would be a chance to note the variations and apparent discrepancies between them, would give students a better knowledge of psychology than they now get from the type of laboratory work currently emphasized, or, than they get from discussions of theories before they are sufficiently familiar with the functioning of human minds to weigh them intelligently. Such study is also necessary to show the relation and importance of other kinds of research to the central problem of mental organization.

The effect of this neglect in the education of psychologists has been to undervalue an instrument which is having a great, though not generally recognized, influence on the advance of individual psychology.

Since academic psychologists are the ones who set up

standards of research and decide whether or not studies are worth while, they, because of their failure to recognize the significance of researches in abnormal psychology, are the ones to be held responsible for retarding progress in this special field.

The only way to eliminate this bottleneck is to see that the education of leaders in psychology includes that which gives the profession its reason for being—a *knowledge of human minds*.

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